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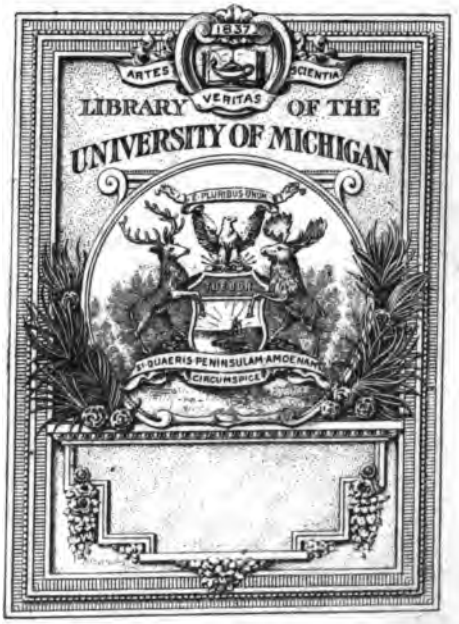
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# PROCEEDINGS

OF

# THE AMERICAN ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE.

## THIRTEENTH MEETING,

HELD AT SPRINGFIELD, MASSACHUSETTS,

AUGUST, 1859.

---

CAMBRIDGE:
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EDITED BY

JOSEPH LOVERING,

*Permanent Secretary.*

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Fairly, Dr. David, Glasgow, Scotland [11].
Farmer, Moses G., Boston, Massachusetts [9].
Farnham, Prof. J. E., Georgetown, Kentucky [7].
Farquhar, W. H., Fair Hill, Montgomerie Co., Maryland [12].
Fay, Rev. Charles, St. Albans, Vermont [11].
Fearing, Dr. E. P., Nantucket, Massachusetts [8].
Fellows, Joseph, Albany, New York [10].
Ferland, J. B. A., Quebec, Canada [11].

Ferrel, William, Nashville, Tennessee [11].
Ferris, Rev. Dr. Isaac, New York, New York [6].
Feuchtwanger, Dr. Louis, New York, New York [11].
Field, Roswell, Greenfield, Massachusetts [13].
Fillmore, Millard, Buffalo, New York [7].
Fisher, Mark, Trenton, New Jersey [10].
Fisher, Prof. George P., New Haven, Connecticut [11].
Fisher, Dr. N. A., Norwich, Connecticut [9].
Fitch, Edward H., Ashtabula, Ohio [11].
*Fitch, Alexander, Hartford, Connecticut [1].
Fitch, O. H., Ashtabula, Ohio [7].
Folsom, George, New York, New York [11].
Foote, Elisha, Seneca Falls, New York [10].
Force, Dr. Charles F., Washington, District of Columbia [12].
Force, Col. Peter, Washington, District of Columbia [4].
Ford, Richard, Columbia, South Carolina [12].
Forshey, Prof. Caleb G., Rutersville, Texas [7].
Fosgate, Dr. Blanchard, Auburn, New York [7].
Foster, J. W., Monson, Massachusetts [1].
Fowle, William B., Boston, Massachusetts [1].
*Fox, Rev. Charles, Grosse Isle, Michigan [7].
Franklin, Danforth, Buffalo, New York [11].
Fraser, Hugh, Montreal, Canada [11].
Fraser, Prof. W., Montreal, Canada [11].
Frazer, Prof. John F., Philadelphia [1].
French, J. H., Syracuse, New York [11].
French, Rev. J. W., West Point, New York [11].
Fristoe, Edward T., Washington, District of Columbia [11].
Frost, Charles C., Brattleboro', Vermont [13].
Frothingham, Rev. Frederick, Portland, Maine [11].
Frothingham, George H., Montreal, Canada [11].

## G.

Gale, Hon. Samuel, Montreal, Canada [11].
Gale, L. D., Washington, District of Columbia [8].
Gardner, James S., Whitestown, New York [10].
Garrigues, Dr. S. S., Philadelphia, Pennsylvania [10].

Garth, Charles, Montreal, Canada [11].
Gavit, John E., New York, New York [1].
Gay, A. M., Charlestown, Massachusetts [10].
*Gay, Dr. Martin, Boston, Massachusetts [1].
Geddes, Charles, Montreal, Canada [11].
Geddings, Prof. E., Charleston, South Carolina [3].
Gibbes, Prof. L. R., Charleston, South Carolina [1].
Gibbes, Prof. Robert W., Columbia, South Carolina [1].
Gibbon, Dr. J. H., Charlotte, North Carolina [3].
Gibbs, Dr. Wolcott, New York, New York [1].
Gibbs, J. Campbell, Montreal, Canada [11].
Gillespie, Prof. W. M., Schenectady, New York [10].
Gilman, Charles, Baltimore, Maryland [12].
Gilman, Daniel C., New Haven, Connecticut [10].
Gilman, Dr. Judson, Baltimore, Maryland [12].
*Gilmor, Robert, Esq., Baltimore, Maryland [1].
Gilmore, Q. A., Jr., U. S. A., New York, New York [13].
Glynn, Com. James, U. S. N., New Haven, Connecticut [1].
Gold, Stephen A., New Haven, Connecticut [13].
Gold, Theodore S., West Cornwall, Connecticut [4].
Goodwin, William F., Concord, New Hampshire [10].
Gould, Dr. Augustus A., Boston, Massachusetts [11].
*Gould, B. A., Boston, Massachusetts [2].
Gould, Dr. B. A., Cambridge, Massachusetts [2].
Graham, Col. James D., U. S. A., Washington, D. C. [1].
Grant, John M., Montreal, Canada [11].
Grant, S. H., New York, New York [11].
Gray, Dr. Henry C., Washington, District of Columbia [12].
Gray, Prof. Alonzo, Brooklyn, New York [13].
Gray, Prof. Asa, Cambridge, Massachusetts [1].
*Gray, Dr. James H., Springfield, Massachusetts [6].
Green, Horace, New York, New York [10].
Green, Dr. Traill, Easton, Pennsylvania [1].
Greene, Dr. Benjamin D., Boston, Massachusetts [1].
Greene, Dr. F. C., East Hampton, Massachusetts [11].
Greene, Samuel, Woonsocket, Rhode Island [9].
Greene, Thomas A., New Bedford, Massachusetts [9].
*Griffith, Dr. Robert E., Philadelphia [1].

Grinnan, A. G., Fredericksburg, Virginia [7].
Groneweg, Lewis, Dayton, Ohio [7].
Grosvenor, A. C., Cincinnati, Ohio [5].
Grunow, Julius, New Haven, Connecticut [10].
Guerin, Thomas, Albany, New York [11].
Guest, William E., Ogdensburg, New York [6].
Gulick, John T., Williamstown, Massachusetts [10].
Gummere, Samuel J., Burlington, New Jersey [7].
Guyot, Prof. Arnold, Princeton, New Jersey [1].

H.

Hackley, Prof. Charles W., New York, New York [4].
Hager, Albert D., Proctorsville, Vermont [11].
Haines, William S., Providence, Rhode Island [9].
Haldeman, Prof. S. S., Columbia, Pennsylvania [1].
*Hale, Dr. Enoch, Boston, Massachusetts [1].
Hall, Prof. James, Albany, New York [1].
Hall, Joel, Athens, Illinois [7].
Hallowell, Benjamin, Alexandria, Virginia [7].
Hamlin, A. C., Bangor, Maine [10].
Hammond, Rev. Charles, Groton, Massachusetts [13].
Hammond, Ogden, Charleston, South Carolina [3].
Hance, Ebenezer, Morrisville, Bucks Co., Pennsylvania [7].
Hand, Thomas J., Baltimore, Maryland [12].
Handy, Rev. Isaac W. K., Portsmouth, Virginia [10].
Hanover, M. D., Springfield, Tennessee [13].
*Hare, Dr. Robert, Philadelphia, Pennsylvania [11].
*Harlan, Prof. Joseph G., Haverford, Pennsylvania [8].
*Harlan, Dr. Richard, Philadelphia, Pennsylvania [1].
Harman, Rev. Henry M., Baltimore, Maryland [12].
Harris, Ira, Albany, New York [11].
*Harris, Dr. Thaddeus W., Cambridge, Massachusetts [1].
Harris, Prof. Chapin A., Baltimore, Maryland [12].
Harrison, B. F., Wallingford, Connecticut [11].
Harrison, Edwin, St. Louis, Missouri [11].
Harrison, Joseph, Jr., Philadelphia, Pennsylvania [12].
*Hart, Simeon, Farmington, Connecticut [1].

Hart, Theodore, Montreal, Canada [11].
Hartshorne, Dr. Henry, Philadelphia, Pennsylvania [12].
Harvey, Hon. Matthew, Concord, New Hampshire [1].
Hathaway, Charles, Delhi, New York [10].
Haven, Samuel F., Worcester, Massachusetts [9].
Hayden, Dr. F. V., Washington, District of Columbia [12].
*Hayden, Dr. H. H., Baltimore, Maryland [1].
Hayward, James, Boston, Massachusetts [1].
Hayes, Dr. Isaac I., Philadelphia, Pennsylvania [12].
Hazard, Rowland, Peace Dale, Rhode Island [9].
Headlam, William, Jr., Albany, New York [11].
Hedrick, B. S., New York, New York [13].
Heneker, William R., Sherbrooke, Canada East [11].
Henry, Prof. Joseph, Washington, District of Columbia [1].
Herring, William A. Duplin, Sampson Co., North Carolina [12].
Hickok, Rev. M. J., Scranton, Pennsylvania [11].
Higgins, Dr. James, Baltimore, Maryland [12].
Hilgard, Eugene W., Oxford, Mississippi [11].
Hilgard, Julius E., Washington, District of Columbia [4].
Hilgard, Dr. T. C., St. Louis, Missouri [8].
Hill, Benjamin, Montreal, Canada [11].
Hill, S. W., Eagle Harbor, Lake Superior [6].
Hill, Pres. Thomas, Yellow Springs, Ohio [3].
Hincks, Rev. William, Toronto, Canada West [11].
Hingston, Dr. W. P., Montreal, Canada [10].
Hitchcock, Charles H., Amherst, Massachusetts [11].
Hitchcock, Prof. Edward, Amherst, Massachusetts [1].
Hitchcock, Edward, Jr., Amherst, Massachusetts [4].
Hoadley, E. S., East Hampton, Massachusetts [13].
Hoblitzell, J. H., Baltimore, Maryland [12].
Hodgins, George, Toronto, Canada West [11].
Hodgson, W. B., Savannah, Georgia [10].
Holland, Joseph B., Westfield, Massachusetts [9].
Holmes, Prof. A. F., Montreal, Canada [11].
Holmes, Benjamin, Montreal, Canada [11].
Holmes, Prof. Francis S., Charleston, South Carolina [3].
Holton, L. H., Montreal, Canada [11].
Holwell, W. A., Quebec, Canada [11].

Homans, J. S., Jr., New York, New York [12].
Homans, Sheppard, New York, New York [10].
Homes, Henry A., Albany, New York [11].
Hopkins, Rev. John H., New York, New York [11].
Horan, E. J., Quebec, Canada [10].
Horsford, Rèv. Benjamin F., Haverhill, Massachusetts [13].
Horsford, Prof. E. N., Cambridge, Massachusetts [i].
Horton, C. V. R., Chaumont, New York [10].
*Horton, Dr. William, Craigville, Orange Co., New York [1].
Hough, Dr. Franklin B., Albany, New York [4].
*Houghton, Dr. Douglas, Detroit, Michigan [1].
Houghton, George F., St. Albans, Vermont [11].
Howard, Dr. Benjamin, Williamstown, Massachusetts [13].
Howard, Dr. R. P., Montreal, Canada [11].
Howe, Prof. Henry A., Montreal, Canada [11].
Howell, Robert, Nichols, Tioga Co., New York [6].
Hubbard, Prof. Oliver P., Hanover, New Hampshire [1].
Humphrey, Frederic, Iowa City, Iowa [11].
Humphreys, Capt. A. A., U. S. A., Washington, D. C. [11].
*Hunt, Freeman, New York, New York [11].
Hunt, George, Providence, Rhode Island [9].
Hunt, Lieut. E. B., U. S. A., Bristol, Rhode Island [2].
Hunt, Thomas S., Montreal, Canada [1].
Hunter, Andrew W., New Orleans, Louisiana [11].
Hunter, George W., New Orleans, Louisiana [11].
Hurd, Rev. Isaac N., Corning, New York [13].
Hurlburt, Dr. J., Hamilton, Canada West [11].
Huse, Prof. Caleb, West Point, New York [13].
Huson, Calvin, Jr., Rochester, New York [11].
Hyatt, James, New York, New York [10].

I.

Inglis, Rev. David, Hamilton, Canada West [11].
*Ives, Thomas P., Providence, Rhode Island [10].

J.

Jack, Prof. W. B., Fredericton, New Brunswick [11].
Jackson, Dr. Charles T., Boston, Massachusetts [1].
James, Prof. Charles S., Lewisburg, Pennsylvania [10].
James, M. P., Cincinnati, Ohio [5].
Janes, D. P., Montreal, Canada [11].
Jenkins, Thornton A., U. S. N., Washington, D. C. [7].
Jenks, J. W. P., Middleborough, Massachusetts [2].
Jennings, N. R., New Orleans, Louisiana [4].
*Johnson, Prof. W. R., Washington, District of Columbia [1].
Johnson, Rev. Lyman H., Elkhorn, Wisconsin [13].
Johnson, William C., Utica, New York [6].
Johnston, A. K., Platteville, Wisconsin [13].
Johnston, Prof. John, Middletown, Connecticut [1].
Johnston, Dr. Christopher, Baltimore, Maryland [12].
*Jones, Lieut. Catesby Ap. R., U. S. N., Washington, D. C. [8].
Jones, Rev. George, U. S. N., Brooklyn, New York [9].
Jones, Dr. Thomas Walter, Montreal, Canada [10].
Joseph, J. H., Montreal, Canada [11].
Joy, Prof. C. A., New York, New York [8].
Judd, Orange, New York, New York [4].

K.

Keefer, Thomas C., Hamilton, Canada West [11].
Keeley, Prof. G. W., Waterville, Maine [1].
Keep, N. C., Boston, Massachusetts [13].
Kelley, Dr. Edwin, Elyria, Ohio [7].
Kemp, Rev. Alexander, Montreal, Canada [11].
Kendall, Joshua, Meadville, Pennsylvania [10].
Kennicott, Robert, West Northfield, Illinois [12].
Kent, Edward N., New York, New York [8].
Kerr, Prof. W. C., Davidson College, North Carolina [10].
Kierskowski, A. E., Montreal, Canada [11].
Kimmel, Col. Anthony, Linganore, Maryland [12].
King, Dr. David, Newport, Rhode Island [13].

King, Hon. Mitchell, Charleston, South Carolina [3].
Kingston, G. T., Toronto, Canada [11].
Kinnear, David, Montreal, Canada [11].
Kirkpatrick, James A., Philadelphia, Pennsylvania [7].
Kirkwood, Daniel, Bloomington, Indiana [7].
Kite, Thomas, Cincinnati, Ohio [5].
Kittredge, Dr. Josiah, South Hadley, Massachusetts [11].
Kloman, Dr. W. C., Baltimore, Maryland [12].
Knox, Rev. J. P., Newtown, Long Island [11].
Kurtz, J. D., Portland, Maine [3].

## L.

Lacklan, Major R., Cincinnati, Ohio [11].
Laflamme, G., Montreal, Canada [11].
Laflamme, Prof. Rodolphe, Montreal, Canada [11].
Lapham, Increase A., Milwaukee, Wisconsin [3].
La Roche, Dr. C. P., Philadelphia, Pennsylvania [12].
La Roche, Dr. R., Philadelphia, Pennsylvania [12].
*Lasel, Prof. Edward, Williamstown, Massachusetts [1].
Latour, L. A. H., Montreal, Canada [11].
Lauderdale, John V., Geneseo, New York [10].
Lawford, Frederick, Montreal, Canada [11].
Lawrence, Prof. Edward A., East Windsor, Connecticut [13].
Lawrence, George N., New York, New York [7].
Lea, Isaac, Philadelphia, Pennsylvania [1].
Lea, Robert M., Aransas, Texas [12].
Leckie, Robert, Montreal, Canada [11].
Le Conte, Dr. John L., Philadelphia, Pennsylvania [1].
Leconte, Prof. John, Columbia, South Carolina [3].
Leconte, Dr. Joseph, Columbia, South Carolina [3].
*Lederer, Baron von, Washington, District of Columbia [1].
Lee, Capt. Thomas J., U. S. A., Ellangowan, Maryland [5].
Leidy, Prof. Joseph, Philadelphia, Pennsylvania [12].
Lesley, J. P., Philadelphia, Pennsylvania [2].
Lesley, Joseph, Jr., Philadelphia, Pennsylvania [8].
Lieber, Oscar M., Columbia, South Carolina [8].
Lincklaen, Ledyard, Cazenovia, New York, New York [1].

Lincoln, Dr. N. S., Washington, District of Columbia [12].
Lindsley, Dr. J. B., Nashville, Tennessee [1].
*Linsley, Rev. James H., Stafford, Connecticut [1].
Little, Weare C., Albany, New York [11].
Litton, Prof. A., St. Louis, Missouri [12].
Locke, Prof. Joseph M., Cincinnati, Ohio [13].
Locke, Dr. Luther F., Nashua, New Hampshire [7].
Logan, Sir William E., Montreal, Canada [1].
Loomis, Prof. Elias, New York, New York [1].
Loomis, Prof. J. R., Lewisburg, Pennsylvania [10].
Loomis, Pres. Charles L., Wilmington, Delaware [9].
Loomis, Dr. Silas L., Washington, District of Columbia [7].
Lord, Rev. John, Stamford, Connecticut [13].
Loosey, Charles F., New York, New York [12].
Lovelace, Dr. P. E. H., Sicily Island, Louisiana [11].
Lovering, Prof. Joseph, Cambridge, Massachusetts [2].
Lunn, William, Montreal, Canada [11].
Lyman, Chester S., New Haven, Connecticut [4].
Lyman, Henry, Montreal, Canada [11].
Lyman, Theodore, Montreal, Canada [11].
Lynch, Rev. Dr. P. N., Charleston, South Carolina [2].
Lyon, Hon. Caleb, Lyonsdale, New York [8].
Lyon, Merrick, Providence, Rhode Island [11].

## M.

MacCallum, Prof. D. C., Montreal, Canada [11].
McAlpine, William J., Albany, New York [6].
M'Cabe, Rev. James D., Mount Washington, Maryland [12].
McCall, Col. George A., Philadelphia, Pennsylvania [7].
McCall, John, Utica, New York [10].
McCauley, James, Elkton, Cecil Co., Maryland [12].
McChesney, J. H., Springfield, Illinois [11].
McClory, Rev. Henry, Warehouse Point, Connecticut [11].
M'Conihe, Hon. Isaac, Troy, New York [4].
McCord, Hon. J. L., Montreal, Canada [11].
McCormick, Richard C., Jr., New York, New York [11].
McCoy, Prof. Amasa, Albany, New York [10].

McCron, Rev. John, Baltimore, Maryland [12].
McElroy, John C., Albany, New York [11].
McFarlan, Henry, Dover, New Jersey [8].
McIlvaine, Rev. J. H., Rochester, New York [11].
McKew, Dr. Dennis, Baltimore, Maryland [12].
McMahon, Mathew, Albany, New York [11].
McNaughton, Dr. James, Albany, New York [4].
McRae, John, Camden, South Carolina [3].
Mackay, T. P., Montreal, Canada [11].
Maclean, Prof. George M., Pittsburg, Pennsylvania [5].
Mahan, Prof. D. H., West Point, New York [9].
Mallett, Prof. J. W., Tuscaloosa, Alabama [10].
Marcy, O., Wilbraham, Massachusetts [10].
*Marsh, Dexter, Esq., Greenfield, Massachusetts [1].
Martin, Prof. W. J., Chapel Hill, North Carolina [12].
Mason, Charles, Burlington, Iowa [11].
*Mather, William W., Columbus, Ohio [1].
Mathieson, Rev. Alexander, Montreal, Canada [11].
Maupin, Prof. S., Charlottesville, Virginia [10].
Mauran, Dr. J., Providence, Rhode Island [2].
Mayhew, Prof. D. P., Ypsilanti, Michigan [13].
Means, Prof. A., Oxford, Georgia [5].
Meech, L. W., Preston, Connecticut [8].
Meek, F. B., Albany, New York [6].
Meigs, Prof. James A., Philadelphia, Pennsylvania [12].
Meilleur, Dr. J. B., Montreal, Canada [11].
Meredith, Edmund P., Toronto, Canada [11].
Merrill, Hubert H., Lebanon, Tennessee [13].
Merryman, Dr. M., Baltimore, Maryland [12].
Merrill, Stephen, Charlestown, Massachusetts [11].
Meyers, Prof. A. M., Baltimore, Maryland [12].
Miles, Prof. Henry H., Lennoxville, Canada East [11].
Millington, Prof. John, Oxford, Mississippi [1].
Minifie, William, Baltimore, Maryland [12].
Mitchel, Prof. O. M., Cincinnati, Ohio [3].
Mitchell, Henry, Nantucket, Massachusetts [10].
Mitchell, Maria, Nantucket, Massachusetts [4].
Mitchell, William, Nantucket, Massachusetts [2].

Mittag, J. F. G., Hagerstown, Maryland [12].
Moffatt, George, Montreal, Canada [11].
Moffatt, J. O., Montreal, Canada [11].
Mondelet, Judge Charles, Montreal, Canada [11].
Montgomerie, Hugh E., London, England [11].
Moore, George H., New York, New York [8].
Montague, Theodore L., Pomeroy, Ohio [13].
Morfit, Dr. Campbell, New York, New York [7].
Morgan, Lewis H., Rochester, New York [10].
Morris, J. R., Houston, Texas [11].
Morris, Rev. John G., Baltimore, Maryland [12].
Morrison, Benjamin F., Nantucket, Massachusetts [13].
Morrow, E. G., Chapel Hill, North Carolina [11].
Morse, Charles M., Waterville, Maine [11].
*Morton, Dr. S. G., Philadelphia, Pennsylvania [1].
Mowat, O., Toronto, Canada [11].
Munger, George G., Rochester, New York [10].
Munro, Col. William, Quebec, Canada [11].
Munroe, Rev. Nathan, Bradford, Massachusetts [6].
Murdoch, Charles N., Baltimore, Maryland [12].
Murdock, John, Baltimore, Maryland [12].
Murphy, Edward, Montreal, Canada [11].
Murray, Alexander, Woodstock, Canada West [11].
Murray, Prof. David, Albany, New York [11].
Myers, Gustavus A., Richmond, Virginia [11].

## N.

Nason, Rev. Elias, Natick, Massachusetts [11].
Nason, Prof. Henry B., Beloit, Wisconsin [13].
Nault, Dr. J. Y., Quebec, Canada [11].
Nelles, Pres. S. S., Victoria College, Canada West [11].
Nelson, J. P., Lewisburg, North Carolina [7].
Nelson, Rev. Cleland K., Annapolis, Maryland [12].
Newcomb, Simon, Cambridge, Massachusetts [13].
Newcomb, Dr. Wesley, Albany, New York [10].
Newell, W. A., Trenton, New Jersey [11].
Newton, Prof. Hubert A., New Haven, Connecticut [6].

Newton, John, Orange Hill, Washington Co., Florida [7].
Newton, Rev. E. H., Cambridge, New York [1].
Nichols, Dr. James R., Haverhill, Massachusetts [7].
Nichols, Prof. John A., New York, New York [10].
*Nicollet, J. N., Washington, District of Columbia [1].
Nolen, George A., New Haven, Connecticut [13].
North, Prof. Edward, Clinton, New York [11].
Norton, Edward, Farmington, Connecticut [13].
*Norton, Prof. J. P., New Haven, Connecticut [1].
Norton, Prof. W. A., New Haven, Connecticut [6].

## O.

*Oakes, William, Ipswich, Massachusetts [1].
Oliver, James Edward, Lynn, Massachusetts [7].
*Olmsted, Alexander F., New Haven, Connecticut [4].
*Olmsted, Prof. Denison, New Haven, Connecticut [1].
*Olmsted, Denison, Jr., New Haven, Connecticut [1].
Opdyke, George, New York, New York [8].
Ordway, John M., Roxbury, Massachusetts [9].
Ormiston, Rev. William, Hamilton, Canada [10].
Osborn, A., Albany, New York [1].
Ormand, J. J., Tuscaloosa, Alabama [10].
Osgood, Rev. Samuel, New York, New York [8].
Osten, Sacken, Baron R. von, Washington, D. C. [10].
Otis, Dr. George A., Jr., Springfield, Massachusetts [10].

## P.

Paine, Cyrus F., Rochester, New York [12].
Painter, Minshall, Lima, Delaware Co., Pennsylvania [7].
Palmer, Dr. A. B., Detroit, Michigan [12].
Paradis, Dr. H. C. A., Montreal, Canada [11].
Parker, Hon. Amasa J., Albany, New York [6].
Parker, Rev. Henry E., Concord, New Hampshire [11].
Parker, Prof. William H., Middlebury, Vermont [10].
*Parkman, Dr. Samuel, Boston, Massachusetts [1].
Parry, Dr. Charles C., Davenport, Iowa [6].

Parsons, Dr. Usher, Providence, Rhode Island [9].
Parvin, Theodore S., Muscatine, Iowa [7].
Paton, George, Galt, Canada West [11].
Paton, John, Toronto, Canada [11].
Patterson, Prof. J. W., Hanover, New Hampshire [11].
Peale, Titian R., Washington, District of Columbia [1].
Peck, Prof. William G., New York, New York [13].
Peckham, Dr. F. H., Providence, Rhode Island [12].
Peirce, Prof. Benjamin, Cambridge, Massachusetts [1].
Peirce, James M., Cambridge, Massachusetts [11].
Perkins, Prof. George R., Utica, New York [1].
Perkins, J. A., Montreal, Canada [11].
*Perry, Com. M. C., New York, New York [10].
Peters, Dr. C. H. F., Clinton, New York [9].
Phelps, Mrs. Almira L., Baltimore, Maryland [13].
Phelps, Charles E., Baltimore, Maryland [13].
Phelps, Prof. William F., Trenton, New Jersey [11].
Pierce, Prof. William M., Geneva, Illinois [10].
Piggot, A. Snowden, Baltimore, Maryland [10].
Pitcher, Dr. Zina, Detroit, Michigan [1].
Pitman, Benn, Cincinnati, Ohio [10].
Plant, I. C., Macon, Georgia [3].
*Plumb, Dr. Ovid, Salisbury, Connecticut [9].
Pope, Prof. Charles A., St. Louis, Missouri [12].
Porter, Prof. Charles H., Albany, New York [13].
Porter, Charles T., New York, New York [13].
Porter, Prof. Thomas C., Lancaster, Pennsylvania [12].
Pourtales, L. F., Washington, District of Columbia [1].
Powers, A. E., Lansingburg, New York [1].
Pratt, J. D., Baltimore, Maryland [12].
Prentice, E. P., Albany, New York [2].
Prescott, Dr. William, Concord, New Hampshire [1].
Prince, W. R., Flushing, Long Island [10].
Pruyn, J. V. L., Albany, New York [1].
Putnam, F. W., Cambridge, Massachusetts [10].
Pynchon, Prof. Thomas R., Hartford, Connecticut [11].

## Q.

Quincy, Edmund, Jr., Dedham, Massachusetts [11].

## R.

Rachmáninow, Dr. J., Kiew, Russia [13].
Ramsay, Andrew C., London, England [11].
Ramsay, J. K., Montreal, Canada [11].
Rankin, Robert G., New York, New York [10].
Rauch, Dr. John H., Burlington, Iowa [11].
Read, D. B., Toronto, Canada [11].
Redfield, Charles B., Albany, New York [11].
Redfield, John H., New York, New York [1].
*Redfield, William C., New York, New York [1].
Reed, Lyman, Baltimore, Maryland [12].
Resor, Jacob, Cincinnati, Ohio [8].
Richards, Z., Washington, District of Columbia [7].
Richards, Rev. William C., Providence, Rhode Island [9].
Richardson, Dr. Horace, Boston, Massachusetts [12].
Ritchie, E. S., Boston, Massachusetts [10].
Robb, Prof. James, Fredericton, New Brunswick [4].
Robertson, Thomas D., Rockford, Illinois [10].
Robertson, Rev. W. H. C., Stamford, Connecticut [11].
Roberts, W. M., Carlisle, Pennsylvania [5].
Robinson, Rev. George C., Cincinnati, Ohio [13].
Robinson, W. B., Toronto, Canada [11].
Rockwell, Alfred P., La Salle, Illinois [10].
Rockwell, John, La Salle, Illinois [11].
Rockwell, John A., Norwich, Connecticut [10].
Rodman, William M., Providence, Rhode Island [9].
Rogers, Prof. Fairman, Philadelphia, Pennsylvania [11].
Rogers, Prof. Henry D., Glasgow, Scotland [12].
*Rogers, Prof. James B., Philadelphia, Pennsylvania [1].
Rogers, Prof. W. B., Boston, Massachusetts [1].
Roosevelt, Clinton, New York, New York [11].

Root, Prof. O., Clinton, New York [13].
Rose, Henry, Montreal, Canada [11].
Roy, Euclide, Montreal, Canada [11].
Ruggles, Prof. William, Washington, District of Columbia [8].
Runkle, J. D., Dedham, Massachusetts [2].
Russell, Andrew, Toronto, Canada [11].
Russell, Archibald, New York, New York [11].
Rutherford, Louis M., Newport, Rhode Island [13].
Ruttan, Dr. Allan, Newburgh, Canada West [11].
Ryan, Thomas, Montreal, Canada [11].
Ryerson, Rev. E., Montreal, Canada [11].

## S.

Safford, Prof. J. M., Lebanon, Tennessee [6].
Safford, Truman H., Cambridge, Massachusetts [13].
Sager, Prof. Abraham, Ann Arbor, Michigan [6].
Sanborn, Francis G., Andover, Massachusetts [13].
Sanger, Dr. W. W., Blackwell Island, New York [10].
Sanderson, Rev. J. E., Montreal, Canada [11].
Sands, Samuel, Baltimore, Maryland [12].
Sargent, Rufus, Auburn, New York [10].
Savage, Thomas S., Pass Christian, Mississippi [10].
Sawyer, A. W., Acadia College, Nova Scotia [13].
Scarborough, George, Owensburg, Kentucky [2].
Schaeffer, Prof. George C., Washington, D. C. [1].
Schaff, Rev. Philip, Mercersburg, Pennsylvania [12].
Schanck, Prof. J. Stilwell, Princeton, New Jersey [4].
Schofield, Prof. J. M., West Point, New York [13].
Schott, Arthur C. V., Washington, District of Columbia [8].
Schott, Charles A., Washington, District of Columbia [8].
Scott, Rev. Joseph, Dunham, Canada East [11].
Scott, Rev. William, Peterborough, Canada West [11].
Scudder, Samuel H., Boston, Massachusetts [13].
Selden, George M., Troy, New York [6].
Seemann, Berthold, London, England [11].
Serrell, Edward W., Greenfield, Massachusetts [13].

Sestini, Prof. Benedict, Washington, District of Columbia [8].
Seward, Hon. William H., Auburn, New York [1].
Seymour, M. H., Montreal, Canada [11].
Shaefer, P. W., Pottsville, Pennsylvania [4].
Shane, J. D., Cincinnati, Ohio [7].
Sheldon, D. H., St. Louis, Missouri [10].
Shelton, E. E., Montreal, Canada [11].
Shepard, Prof. C. U., New Haven, Connecticut [4].
Sheppard, William, Drummondville, Canada [11].
Sherwin, Thomas, Dedham, Massachusetts [11].
Shotwell, Samuel L., Macedon, New York [11].
Shumard, Dr. B. F., Austin, Texas [7].
Sias, Solomon, Bonnan, Fannin Co., Texas [10].
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio [6].
Silliman, Prof. Benjamin, New Haven, Connecticut [1].
Silliman, Prof. Benjamin, Jr., New Haven, Connecticut [1].
Skilton, Dr. Avery J., Troy, New York [6].
Skinner, Dr. George W., Little Falls, New York [10].
Slack, J. H., Philadelphia, Pennsylvania [12].
Smallwood, Prof. Charles, St. Martin, Isle Jesus, C. E. [7].
Smith, Prof. Augustus W., Annapolis, Maryland [4].
Smith, Capt. E. K., U. S. A. [8].
Smith, Prof. Francis H., University of Virginia [9].
Smith, George, Upper Darby, Delaware Co., Pennsylvania [7].
Smith, Dr. J. B., New York, New York [7].
Smith, James Y., Providence, Rhode Island [9].
*Smith, J. V., Cincinnati, Ohio [5].
Smith, John, Montreal, Canada [11].
Smith, Dr. Lyndon A., Newark, New Jersey [9].
Smith, Sanderson, New York, New York [9].
Smith, Hamilton L., Gambier, Ohio [5].
Smith, Spencer, St. Louis, Missouri [11].
Snell, Prof. Eben S., Amherst, Massachusetts [2].
Snodgrass, Rev. William, Montreal, Canada [11].
Snow, Charles B., Washington, District of Columbia [8].
Snow, Dr. Edwin M., Providence, Rhode Island [9].
Sola, Rev. Dr. A., Montreal, Canada [11].

Sparks, Jared, Cambridge, Massachusetts [2].
Spear, C. V., Pittsfield, Massachusetts [10].
Spink, William, Toronto, Canada [11].
Sprague, Daniel J., South Orange, New Jersey [11].
Sprague, Rev. Daniel G., South Orange, New Jersey [11].
Spring, Dr. Charles H., Holyoke, Massachusetts [13].
Staley, Rev. George L., Mt. Washington, Maryland [12].
Stanard, Benjamin A., Cleveland, Ohio [6].
Starr, William, Ceresco, Wisconsin [10].
Stearns, Josiah A., Boston, Massachusetts [10].
Stearns, Prof. William F., Oxford, Mississippi [13].
Steiner, Dr. Lewis H., Baltimore, Maryland [7].
Sterling, Prof. J. W., Madison, Wisconsin [13].
Stevens, Prof. M. C., Richmond, Indiana [9].
Stevens, Dr. Robert P., North Egremont, Massachusetts [7].
Stewart, Prof. William M., Clarksville, Tennessee [7].
Stimpson, William, Washington, D. C. [12].
Stone, Rev. Edwin M., Providence, Rhode Island [9].
Storer, Dr. D. H., Boston, Massachusetts [1].
Storer, Frank H., Boston, Massachusetts [13].
Streeter, S. F., Baltimore, Maryland [11].
Stuart, Prof. A. P. S., Acadia, Nova Scotia [11].
Sullivant, William S., Columbus, Ohio [7].
Sutherland, Prof. William, Montreal, Canada [6].
Swallow, Prof. G. C., Columbia, Missouri [10].
Swinburne, John, Albany, New York [6].

## T.

Talcott, Andrew, Cincinnati, Ohio [7].
*Tallmadge, Hon. James, New York, New York [1].
Tatlock, Prof. John, Williamstown, Massachusetts [10].
Tayler, John W., Wampsville, New York [10].
*Taylor, Richard C., Philadelphia, Pennsylvania [1].
Taylor, J. W., Montreal, Canada [11].
Taylor, Rev. William, Montreal, Canada [11].
Taylor, Thomas M., Montreal, Canada [11].

Taylor, W. I., Worcester Co., Maryland [12].
*Teschemacher, J. E., Boston, Massachusetts [1].
Tevis, Robert C., Shelbyville, Kentucky [5].
Thickstun, T. F., Chatfield, Minnesota [11].
Thomas, Richard, Montreal, Canada [11].
Thomas, William A., Kingston, Massachusetts [10].
Thompson, Dr. Alexander, Aurora, New York [6].
Thompson, Aaron R., New York, New York [1].
Thompson, H. C., Chapel Hill, North Carolina [13].
Thompson, Dr. John A., Geneva, New York [10].
*Thompson, Rev. Z., Burlington, Vermont [1].
Thomson, Rev. John, New York, New York [11].
Thurber, Isaac, Providence, Rhode Island [9].
Tilghman, Gen. Tench, Oxford, Maryland [12].
Tobey, Dr. Samuel B., Providence, Rhode Island [9].
Tolderoy, Dr. James B., Fredericton, New Brunswick [11].
Torrey, Dr. John, New York, New York [1].
Torrey, Prof. Joseph, Burlington, Vermont [2].
Totten, Gen. J. G., U. S. A., Washington, D. C. [1].
Townsend, Hon. Franklin, Albany, New York [4].
*Townsend, John K., Philadelphia, Pennsylvania [1].
Townsend, Robert, Albany, New York [9].
Treadwell, C. P., L'Original, Canada West [11].
*Troost, Dr. Gerard, Nashville, Tennessee [1].
Trowbridge, Prof. W. P., Washington, District of Columbia [10].
Trudeau, Alexis, Montreal, Canada [11].
*Tuomey, Prof. M., Tuscaloosa, Alabama [1].
Turnbull, Lawrence, Philadelphia, Pennsylvania [10].
Tuthill, Dr. Franklin, New York, New York [8].
Tuttle, David K., University of Virginia [12].
*Tyler, Rev. Edward R., New Haven, Connecticut [1].
Tyler, Hon. P. B., Springfield, Massachusetts [13].
Tyler, Ransom H., Fulton, New York [10].
Tyler, Robert S., Montreal, Canada [11].
Tyson, Philip T., Baltimore, Maryland [12].

## U.

Uhler, P. R., Baltimore, Maryland [12].

## V.

Vail, Prof. Hugh, Haverford, Pennsylvania [8].
Van Benschoten, James C., Oxford, Chenango Co., New York [12].
Vancleve, John W., Dayton, Ohio [1].
Van Pelt, Dr. William, Williamsville, Erie Co., New York [7].
*Vanuxem, Lardner, Bristol, Pennsylvania [1].
Van Vleck, J. M., Middletown, Connecticut [9].
Vaux, William S., Philadelphia, Pennsylvania [1].

## W.

Wadsworth, Charles F., Genesee, New York [11].
Wadsworth, James S., Genesee, New York [2].
Wagner, Tobias, Philadelphia, Pennsylvania [9].
Walker, Joseph, Oxford, New York [10].
*Walker, Sears C., Washington, D. C. [1].
*Walker, Hon. Timothy, Cincinnati, Ohio [4].
Wallbridge, T. C., Belleville, Canada West [11].
Ward, Henry A., Rochester, New York [13].
Warder, Dr. J. A., Cincinnati, Ohio [4].
Warner, H. G., Rochester, New York [11].
Warren, G. K., U. S. A. Top. Engineers [12].
*Warren, Dr. John C., Boston, Massachusetts [1].
Watertown, Charles, Wakefield, England [1].
Watson, Prof. James C., Ann Arbor, Michigan [13].
Watson, William, Cambridge, Massachusetts [12].
*Webster, H. B., Albany, New York [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Albany, New York [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Weed, Monroe, Wyoming, New York [6].

Welch, John, Newark, New Jersey [10].
West, Charles E., Buffalo, New York [1].
Wethered, Charles E., Baltimore, Maryland [12].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Wheatland, Richard H., Salem, Massachusetts [13].
Wheatley, Charles M., New York, New York [1].
Wheeler, Dr. T. B., Montreal, Canada [11].
Wheildon, W. W., Charlestown, Massachusetts [13].
Whipple, A. B., Nantucket, Massachusetts [10].
Whipple, J. E., Lansingburg, New York [10].
White, Aaron, Cazenovia, New York [10].
White, Charles, Crawfordsville, Indiana [10].
White, Rev. Robert J. P., Chambly, Canada East [11].
Whitney, Asa, Philadelphia, Pennsylvania [1].
Whitney, H. H., Montreal, Canada [11].
Whitney, J. D., Northampton, Massachusetts [1].
Whitney, John R., Philadelphia, Pennsylvania [11].
Whitney, Prof. William D., New Haven, Connecticut [12].
Whittlesey, Charles, Cleveland, Ohio [1].
Whittlesey, Charles C., St. Louis, Missouri [11].
Wilder, Alexander, New York, New York [10].
Wilgress, George, Montreal, Canada [11].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Wilkes, Rev. Henry, Montreal, Canada [11].
Willard, Samuel D., Cayuga, New York [10].
Williams, Dr. Henry W., Boston, Massachusetts [11].
Williams, Samuel Wells, Canton, China [10].
Williams, Thomas H., Portsmouth, Virginia [12].
Williams, William W., New York, New York [12].
Williamson, Prof. James, Kingston, Canada [11].
Williamson, Capt. R. S., U. S. A., San Francisco, Cal. [12].
Wilson, Prof. Daniel, Toronto, Canada [10].
Wilson, Prof. W. C., Carlisle, Pennsylvania [12].
Winchell, Prof. Alexander, Ann Arbor, Michigan [3].
Winslow, C. F., Newton, Massachusetts [10].
Woodall, John W., Scarborough, England [11].
Woodbridge, George A., Nashville, Tennessee [10].

*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodman, John S., Hanover, New Hampshire [11].
Worthen, A. H., Springfield, Illinois [5].
Wright, Charles, Wethersfield, Connecticut [10].
Wright, Chauncey, Cambridge, Massachusetts [9].
*Wright, Dr. John, Troy, New York [1].
Wurtele, Louis C., Lennoxville, Canada East [11].
Wurtz, Prof. Henry, Washington, District of Columbia [10].
Wyman, Prof. Jeffries, Cambridge, Massachusetts [10].
Wyman, Dr. Morrill, Cambridge, Massachusetts [11].
Wynne, Thomas H., Richmond, Virginia [8].

## Y.

Youmans, E. L., Saratoga Springs, New York [6].
*Young, Prof. Ira, Hanover, New Hampshire [7].
Young, Hon. John, Montreal, Canada [11].
Young, William H., Baltimore, Maryland [12].

The above list contains Nine Hundred and Forty names, of which Seventy-Eight are of deceased members.

# MEMBERS ELECTED

AT

# THE SPRINGFIELD MEETING.

*Bardwell, Prof. F. W., Yellow Springs, Ohio.
Barker, George F., Boston, Mass.
Barratt, Dr. Joseph, Middletown, Conn.
*Bebb, M. S., Rockford, Illinois.
Bentley, Cyrus, Chicago, Illinois.
*Blair, Prof. F. C., Lebanon, Illinois.
*Boynton, Prof. Edward C., Oxford, Mississippi.
Briggs, A. D., Springfield, Mass.
*Butler, Prof. James D., Madison, Wisconsin.

Churchill, Marlborough, Sing Sing, New York.
*Clark, H. J., Cambridge, Mass.
Clarke, Dr. A. B., Holyoke, Mass.
*Cobleigh, Pres. Nelson E., Lebanon, Illinois.
†Coffin, C. C., Malden, Mass.
Coryell, Thomas D., Madison, Wisconsin.
*Cummings, Pres. Joseph, Middletown, Conn.
Curtiss, Charles W., Evanston, Illinois.
Dow, Prof. George W., Chicago, Illinois.
*Eaton, Daniel C., New Haven, Conn.
Eastwood, George, Cambridge, Mass.
Eggleston, Rev. N. H., Madison, Wisconsin.
Ely, George H., Rochester, New York.

Field, Roswell, Greenfield, Mass.
Frost, Charles C., Brattleboro', Vermont.

*Gillmore, Q. A., New York, New York.
Gold, Stephen A., New Haven, Conn.
*Gray, Prof. Alonzo, Brooklyn, New York.

Hammond, Rev. Charles, Grafton, Mass.
Hanover, M. D., Springfield, Tennessee.
*Hedrich, B. S., New York, New York.
‡Hoadley, E. S., East Hampton, Mass.
Horsford, Rev. B. T., Haverhill, Mass.
Howard, Dr. Benjamin, Williamstown, Mass.
Hurd, Rev. Isaac N., Corning, New York.
*Huse, Prof. Caleb, West Point, New York.
Johnson, Rev. Lyman H., Elkhorn, Wisconsin.

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* Those marked with an asterisk paid the assessment and signed the Constitution, without being formally elected.

† Have not been heard from.

‡ Did not sign the Constitution.

Johnston, A. K., Platteville, Wisconsin.

*Keep, N. C., Boston, Mass.
‡King, Dr. David, Newport, Rhode Island.

*Lawrence, Prof. Edward A., East Windsor.
*Locke, Prof. Joseph M., Cincinnati, Ohio.
Lord, Rev. John, Stamford, Conn.

*Mayhew, Prof. D. P., Ypsilanti, Michigan.
†Macy, Alfred, Nantucket, Mass.
Merrill, Hubert H., Lebanon, Tennessee.
Montague, Theodore L., Pomeroy, Ohio.
Morrison, Benjamin L., Nantucket, Mass.

*Nason, Prof. Henry B., Beloit, Wisconsin.
‡Newcomb, Simon, Cambridge, Mass.
Nolen, George A., New Haven, Conn.
Norton, Edward, Farmington, Conn.

*Peck, Prof. William G., New York, New York.
‡Phelps, Mrs. Almira L., Baltimore, Maryland.
Phelps, Charles E., Baltimore, Maryland.
†Poole, Henry W., Boston, Mass.
Porter, Prof. Charles H., Albany, New York.
Porter, Charles T., New York, New York.
†Powell, Samuel, Newport, Rhode Island.

Rachmáninow, Dr. J., Kiew, Russia.
Robinson, Rev. George C., Cincinnati, Ohio.
*Root, Prof. O., Clinton, New York.
‡Rutherford, Lewis M., New York, New York.

Safford, Truman H., Cambridge, Mass.
Sanborn, Francis G., Andover, Mass.
*Sawyer, A. W., Acadia College, Nova Scotia.
*Schofield, Prof. J. M., West Point, New York.
*Scudder, Samuel H., Boston, Mass.
*Serrell, Edward W., Greenfield, Mass.
Spring, Dr. Charles H., Holyoke, Mass.
*Stearns, Prof. William F., Oxford, Mississippi.
*Sterling, J. W., Madison, Wisconsin.
*Storer, Frank H., Boston, Mass.

†Thompson, H. C., Chapel Hill, North Carolina.
‡Tyler, Hon. P. B., Springfield, Mass.

Ward, Henry A., Rochester, New York.
†Waterbury, Dr. Robert L., New York, New York.
*Watson, James C., Ann Arbor, Michigan.
*Wheatland, Richard H., Salem, Mass.
Wheildon, W. W., Charlestown, Mass.
†Wilber, Prof. C. D., Aurora, Illinois.
†Williams, Dr. Mather, Syracuse, New York.

The following paid the assessment without signing the Constitution, or being formally elected.

Goodrich, C. S.

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The following signed the Constitution, and paid the assessment, but was not formally elected.

Bruce, C. E., Ashtabula, Ohio.

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The following have voluntarily withdrawn from the Association, or declined their election.

David Dyer.
J. H. Fairchild.
Joseph Mackay.
Owen Mason.
M. F. Maury.
H. W. Ripley.

# ADDRESS

OF

## PROFESSOR ALEXIS CASWELL,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE YEAR 1858,

ON RETIRING FROM THE DUTIES OF PRESIDENT.

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MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: —

IT sometimes happens that one finds himself in a position which requires an explanation, or even an apology. Such is my own case this evening. The duty which devolves upon me more properly belongs to another. Your Constitution provides for a Retiring Address from your President. He becomes, for the time being, your official adviser, — the exponent of your views. This is one of the objects contemplated in calling him to the presidency. Your Vice-President holds a somewhat different relation to the body, and may not, without seeming arrogance, take upon himself the special functions of his official superior.

My own predilections and sense of fitness would have precluded my appearance before you in this official capacity.

Hitherto, on these occasions, you have listened to the accredited representatives and leaders of American science, each of whom might well apply to himself the motto, "Quorum magna pars fui." I am but an humble laborer in the ranks. But I am here at your command, which, having for its object the advancement of science, is not lightly to be set aside. I should be unworthy of the confidence which you have reposed in me did I not endeavor to comply with your wishes in giving to our proceedings that completeness which the constitution contemplates. But in doing this, if I shall fail to offer any thing worthy of your attention, I beg you to bear in mind that it is not a failure of my own seeking. When I have done what I can, I shall take shelter in a divided responsibility.

Permitted to convene on this, our Thirteenth Annual Meeting, it is fit that we should, as a first duty, pay a passing tribute of respect to the memory of those who have left us. It is well that we should pause even in the grave and urgent pursuits of life to ponder on its brevity.

Since our last meeting, several who have been accustomed to participate in our proceedings, have ceased to be numbered with the living. Prof. Ira Young, of Dartmouth College; Prof. William M. Mather, of the University of Ohio, and at the time of his death the acting President; and Prof. Denison Olmsted, of Yale College, have been borne to their long homes. They were all widely known in Academic circles.

Prof. Young was born in the town of Lebanon, New Hampshire, May 23, 1801. His father was by trade a carpenter, which trade the son also followed till he had attained his majority. He early manifested mechanical ingenuity and an ardent love of knowledge. On arriving at the age of twenty-one, he commenced a course of study preparatory to entering college; which, with his diligence and ability, was soon completed. He entered Dartmouth College, and, having completed the academic course, graduated with distinction in 1828. Like many distinguished graduates of our colleges, he defrayed the expenses of his education mainly by the fruits of his own in-

dustry. He was soon called to the post of Tutor in the college, and after two years of service in that capacity, was elected to the Professorship of Natural Philosophy and Astronomy, which chair he filled with marked ability and success, till the time of his death, Sept. 13, 1858, — a period of twenty-five years. He was a man of exact knowledge, of great firmness, of inflexible integrity, and of exemplary Christian piety. He was well versed in all the subjects pertaining to his department, and was, in the opinion of those most familiar with him and most competent to judge, an admirable teacher. He died in the fifty-eighth year of his age.

Prof. Mather was a native of Middlesex county, in the State of Connecticut. The date of his birth has not fallen under my notice. His name appears in the list of graduates of the United States Military Academy, at West Point, for 1828. He entered the army, but in 1829 was appointed Assistant Professor of Chemistry, Mineralogy, and Geology in the Academy, and continued to discharge the duties of that post for a period of six years. He resigned his commission in the army in 1836, and was elected to the professorship of Chemistry in the University of Louisiana, which place he held but for a short period. He was connected in succession with the geological surveys of the States of New York, Ohio, and Kentucky. His final report on the geology of the first district in New York was published in a large quarto volume in 1823. This is regarded as his most important original work, and in the opinion of competent judges, bears "honorable testimony to his ability and accuracy as an observer in this department of nature." * He also published valuable reports of his labors in connection with the surveys of Ohio and Kentucky. From 1847 to the time of his death, Feb. 29, 1859, he was Professor of Natural Sciences in the University of Ohio, and for some time previous to his death acting President of that institution. He was a valuable contributor for many years to the American Journal

* American Journal of Science, Vol. XXVII. p. 452.

of Science, in the departments of Chemistry, Mineralogy, and Geology.

For the facts connected with the life of Prof. Olmsted I am indebted mainly to a discourse commemorative of his life and services, delivered in the college chapel at New Haven, shortly after his death, by President Woolsey.

From this it appears that he was born in East Hartford on the 18th of June, 1791. He was a descendant of James Olmsted, one of the first settlers of the colony of Connecticut. Bereaved of his father while but an infant, his youthful training was under the direction of his pious and most excellent mother. He gave early indications of a love of books, and diligently availed himself of such slender means of instruction as the common schools of the place and the moderate resources of his family could afford. With the aid and encouragement of a few friends who became interested in his youthful promise, he prepared himself for admission to Yale College in 1809, where he was graduated with distinction in 1813. During his collegiate course he indicated no marked preference for any particular branch of study. If there was any department in which he might be said to be distinguished, it appears to have been that of literature. "He excelled in writing," says President Woolsey, "and cultivated a taste for belles-lettres and poetry." On leaving college, he taught for a while a select school for boys, and was then appointed a tutor in the institution; where, in addition to the duties of his tutorship, he commenced the study of theology, under the supervision of that distinguished teacher and theologian, Dr. Timothy Dwight. During this same year, Mr. Olmsted was appointed Professor of Chemistry in the University of North Carolina, the sciences of geology and mineralogy being included in the professorship. While connected with that university he was invited by the State to undertake a survey of its geology and mineral treasures. A report of these labors was published in 1824–5, under the direction of the State Board of Agriculture. After a period of seven years' service at Chapel Hill, he accepted, in

1825, the appointment of Professor of Mathematics, Natural Philosophy, and Astronomy in his Alma Mater. For thirty-four years, with the utmost diligence and fidelity, he fulfilled the responsible duties of this station, with the exception that the department of Mathematics was after a few years constituted a separate professorship, and placed in other hands. It may be remarked in this connection, that his tastes and his early professional studies, did not lead him to the cultivation of the higher branches of the Mathematics. Throughout the long period of his professional life he sustained the reputation of an excellent teacher, specially apt in the clear and skilful presentation of whatever subject he had occasion to treat before his classes. He was the author of several text-books for colleges, which have passed through many editions. He also gave to the public several popular compends of science, designed for more general use. His text-books for colleges were his Natural Philosophy and his Astronomy. In both of these works he has restricted himself to the more elementary portions of the sciences, and has employed exclusively the geometrical methods in distinction from the analytical. It seems to have been with him through life a leading object to make science popular, and this perhaps determined him in the particular mode of presenting it, deeming the geometrical method, of the two, the more readily understood. However true this may be, — and for beginners it undoubtedly is true, — the calculus, since the time of Newton, has been the great instrument for developing, in all their extent, the physico-mathematical sciences, and cannot be too strongly commended to those who would prepare themselves for independent and aggressive research.

In addition to his scientific and more popular books, Prof. Olmsted was a very frequent contributor to the pages of the American Journal of Science and Art. He was also an early and efficient friend of popular education, and was among the first to urge the importance of normal schools for the education of teachers.

The extraordinary shower of meteors during the night of

Nov. 12, 1833, drew the attention of Prof. Olmsted to the investigation of that subject. After a very laborious inquiry, both historical and scientific, he came to the conclusion that these meteors were portions of the extreme parts of a nebulous body which revolves around the sun interior to that of the earth, but little inclined to the plane of the ecliptic; having its aphelion near the earth's path, and having a periodic time of 182 days nearly.* These researches attracted the attention of the public at home and abroad, and received the commendation of several of the most eminent astronomers of Europe. The explanation here given unites, perhaps, a larger number of suffrages than any other, and yet is hardly to be considered as one of the established facts of science.

The elaborate memoir of Prof. Olmsted, entitled, "On the Recent Secular Period of the Aurora Borealis," published by the Smithsonian Institution, may justly be regarded as one of the most valuable papers which have been given to the public on the perplexed subject of the Northern Lights.

Few men have been called to drink more deeply of the bitter cup of affliction than our deceased friend and associate. Between the years of 1844 and 1852, he buried four dutiful and promising sons, — all graduates of Yale College. Their mother had died before them. These heavy bereavements he bore with singular Christian fortitude and resignation. As a citizen, teacher, and friend, he was universally beloved. I should fail in doing justice to his memory did I not quote a single sentence, illustrative of his character, written a few days before his discease. "In view of the uncertain issue of this sickness," he writes, "I desire humbly to cast myself upon God, humbly to implore his forgiveness of my sins through Jesus Christ, and to express a cheerful hope that, should I be called away, it will be to my heavenly Father's house."

We now pass to other considerations appropriate to this occasion.

* Woolsey's Discourse, p. 17.

It is not expected that this Address will take the form of a scientific paper. It embraces a wider scope and more varied topics. And yet it is hoped that it will not, on that account, be the less pertinent to the general purposes of our meeting.

The object of this Association is the advancement of science. Here two things are implied: first, that science is an object worthy of our pursuit; and, secondly, that combined efforts are necessary to its progress. A few moments' consideration of these points may not be without interest.

1. To say that science is worthy of our pursuit, may at first seem to be a mere waste of words, — the statement of a proposition to which every one assents. But what do we mean by science? Is it the method of converting the products of the fields and forests into articles of commercial value? Does it specially embrace those labor-saving machines by which one man does the work of twenty or an hundred? Is it chiefly found in the immense accumulation of mechanical force by which ponderous masses of iron and stone and timber are handled and tossed about as if they were mere playthings in the hands of giants? Certain it is that science has largely contributed to these results. But results like these by no means limit the value and range of science. These are examples of its immediate ability. But it has claims to our consideration beyond its immediate utility. The high and very just appreciation of what is termed practical and useful has, no doubt, in many instances, operated to the injury of what is termed speculative and theoretical. Nor is this singular. The random speculations and the fallacious theories with which the world abounds, under the pretence of improvements and progress, have contributed in the general mind to depreciate all scientific results, I will not say, which are not susceptible of practicable applications (for of this we cannot judge), but which have not yet been applied to practice. There is everywhere a pressing demand for what is practical, and often a narrow utilitarianism, which looks with suspicion, and sometimes with contempt, upon the profoundest speculations of

science, because they do not pay back to the laborer the wages of his hire in marketable value. The scientist, no less than the severest utilitarian, rejoices in every contribution of science to the arts, but he does not admit that the whole value of science is to be measured by any present applications. He puts in a demurrer to the conclusion that those portions of it are useless of which we do not now see the utility. The use may be beyond our present sphere of vision, or, if coming within our cognizance, it may not admit of comparison with any standard of measure. Unlike the precious gem, which has an exchangeable but no intrinsic value, science bears no price in the market. It transcends all ideas of comparison and exchange. Its high utility lies in the breadth and dignity and sublime grandeur which it gives to the human mind.

In the succession of geological strata, buried deep in the earth, with here and there an upheaval which brings them into view, as in the computed periods of astronomical perturbations, what vast cycles of time are brought within the grasp of the understanding? It is not merely the naked, isolated conception of an immense duration, but a distinct and orderly succession of events, which makes up the physical history of that duration.

In the narrow sense of utility, it may be of little use to know what relation or fellowship exists between the two companions of a minute double star, — minute to us, but of vast intrinsic splendor; — but what educated mind can be indifferent to the fact that the identical law of gravity which determines the fall of an apple, binds those stars to each other, and compels them, through vast cycles of time and almost boundless space, to trace their elliptic orbits? That, to our apprehension, is the grandest law of nature. It controls the material universe, and heralds from sphere to sphere, in endless succession, the order and harmony which emanate from the Creator's will. It is thus, under the guidance of science, that our conceptions of law and order, of space and time, lead us to the Infinite. We look at the starry hosts, — myriads though

they be, — and ask, are there not more? Is it in the lowest degree probable that the material world is bounded by the utmost limits of our telescopic vision? The universal dominion of the same law, the final stability which prevails over inevitable and endless perturbations, mark the universe as a system of exquisite and most perfect mechanism. It is the work of one Intelligence, and that one Intelligence is infinite. I cannot but think that the profoundest knowledge of nature leads, logically, to this conclusion. And this is one of the grandest utilities of science. It raises the human mind to the intelligent contemplation of the Deity.

But this is not all. The ministry of science stops not here. I go further and maintain that a profound knowledge of nature, in her laws and adaptations and vastness, puts us in possession of the truest and best means of weighing the evidence of a divine revelation, and suggests many analogies in its defence. I shall not hesitate, in this presence, to give utterance to my own profound conviction that all true science is in harmony with the Bible, rightly interpreted. Any seeming discrepancy which baffles the resources of ingenuity to reconcile is but the varying ripple on the mighty swell of the ocean, whose exact form no power of analysis can express, and no skill of the pencil sketch. It is but the fleecy cloud which dims a little patch of the deep blue sky, or the smallest visible spot which can be discerned on the broad, blazing disc of the sun, which would not be seen at all but for the splendor of its surroundings.

Science is not unfriendly to religion. True it is that some men, distinguished for science, have appeared to disregard the obligations of religion, and others have equally disregarded the obligations of morality. But science cannot be held responsible, nor ought it to suffer reproach for delinquencies like these. Copernicus was a grave and devoted priest of religion, giving his energies, first to the cure of souls, then to the ministrations of science. Kepler, it is said, seldom entered upon an important investigation without first seeking, by prayer,

the wisdom which is from above. Newton was a devout Christian, scarcely less distinguished for his piety than for his unrivalled power of mathematical analysis and his rare skill in experimental research. Examples like these, of which the annals of philosophy are richly stored, while they prove nothing in respect to religion, may well serve to exonerate science from the charge of infidel tendencies.

I have adverted to this topic for the purpose of glancing at the higher ends of science, and specially for the purpose of relieving the fears of any who may look upon the bold and fearless advance of physical investigations as ominous of evil to the interests of religion. We participate in no such fear. We wish explicitly to exonerate this Association from all suspicion of undermining, or in any manner weakening, the foundations of that faith which an apostle says was once delivered to the saints. We cannot admit the opinion, that any progress in science will ever operate to the disparagement of that devout homage which we all owe to Him in whose hand our breath is and whose are all our ways. Science, on the contrary, lends its sanction and adds the weight of its authority to the sublime teachings of revelation. Such are its ends, over and beyond its immediate practical utilities, and such are its claims upon our service.

2. We may now say, that combined efforts are necessary to its further progress.

The mathematical sciences grow by a development from within; the physical mainly by accretions from without. To the progress of pure analysis it might seem that there was little opportunity for the coöperation of different minds, — that one master-spirit must lead the way, and all others must follow. The coöperation here is not by simultaneous, so much as by successive, labors. The ancient geometers laid the foundation; the modern have built the superstructure. Each successive laborer has resumed the work where his predecessor left it. Each has added his link to the golden chain. And yet even here so varied and distinct are the fields of analytical

research, that the demand for labor is always fully equal to the supply.

The physical sciences advance by observation, by the accumulation of fact, by experimental research and induction. Here the simultaneous coöperation of many laborers is often indispensable. In the science of Meteorology, for example (if in its present state it can properly be called a science), what an accumulation of observations must be made, simultaneous, widely extended, and long continued, before the work of discussion and critical analysis can hope to develop minutely the laws of atmospheric change. These changes, however seemingly capricious, are doubtless as much in accordance with fixed laws as the planetary orbits. But whether the complicated elements which enter into them ever can be grasped and reduced to form, so as to give to the science that completeness which belongs to Astronomy, may well be doubted. That results most important to agriculture, to commerce, and to the sanitary condition of mankind may be reached, is no longer problematical. It may be regarded as one of the certainties of future progress. But these results must be based upon the comparison and discussion of a wide range of observation. Much is to be hoped in this respect from the system of observations organized by the Smithsonian Institution in this country, in connection with similar organizations in England, France, and Germany. The same extended coöperation is required for the advancement of Geology, Mineralogy, Terrestrial Magnetism, Botany, Zoölogy, and indeed of all the departments of Natural History.

What we want, then, and what we aim at, in this Association, is to render the scientific resources of the country available. The coöperative agencies are numberless. Every eye that can see, every ear that can hear, every hand that can record, and every intellect that can analyze and combine, may contribute to the grand result. We invite the widest coöperation. The wisdom of this body will point out the most productive fields of labor, and, as far as possible, suggest the best methods of research.

But in order to gather around us, and bring into active co-operation the scientific talent of the country, we must work with a true devotion to science. By our practical wisdom, our friendly criticism, our impartial justice, our magnanimity and self-sacrifice, we must show that we deserve the confidence of an enlightened public.

In glancing at the field before us, I must limit my remarks to a single department, — that of Astronomy, — to which my own attention has been more particularly given.

It has long been the boast of Astronomy, that it is the oldest and most perfect of the sciences. But how can we predicate perfection of a science that is in a state of rapid advancement? Astronomy at the present time must be regarded as one of the most progressive of the sciences. One or two illustrations of its progress will not be out of place. Tycho Brahè was a great astronomer. His magnificent observatory has a world wide celebrity. And yet it was an observatory without a telescope. Compare the pinnules and coarse graduations of his instruments with the refinements of modern telescopes, circles, and micrometers, and you have an impressive idea of progress. By a laborious comparison of Tycho's observations, Kepler arrived at the conclusion that the planets revolve in elliptical orbits. By the law of gravitation it is now shown that those elliptical orbits are subject to perpetual perturbations, and the minute changes which they must severally undergo for ages to come are pointed out with almost unerring precision. These perturbations themselves become, in the hands of the physical astronomer, unequivocal exponents of the condition of the system. They open up new vistas into space, through which his far-reaching analysis catches the gleams of coming light. The discovery of the planet Neptune will ever constitute a brilliant epoch in the annals of Astronomy, and place the names of Leverrier and Adams in the foremost rank of geometers. The planet Uranus was known to be subject to slight irregularities of motion. Conjecture had for some time assigned the cause to be the attraction of an unknown planet. Analysis in

the hands of these geometers confirmed the existence of such planet, and pointed to its place in the far off depths of ether. And on turning the telescope to the spot, behold there it was. Such an achievement belongs to the ninteenth century, and was scarcely possible at an earlier period. Without accurate measurements the irregularities of Uranus could not have been detected; without that analysis, itself one of the noblest monuments of the human intellect, the place of the unknown planet could not have been conjectured.

As a further illustration, let me refer to the recent labors of Hansen in perfecting the Lunar Tables. The irregularities of the moon's motions, owing to the perturbation of the sun and planets, have been the subject of perplexity and anxious inquiry with astronomers from the time of Hipparchus. No tables could be constructed which would assign the moon's place with accuracy for any long period. So great was the difficulty of the problem that Euler despaired of ever being able to reduce the error in the computed place of that body, to any very narrow limits. Hansen undertook the reëxamination of the Lunar Theory, as based upon the best modern observations, and particularly upon those of the Greenwich Observatory. His profound knowledge of the theory of perturbations enabled him to detect two inequalities of considerable magnitude, due to the action of Venus; which being incorporated in the tables relieves the subject almost entirely of embarrassment, and assigns the moon's place for any epoch with all the precision required by the present state of the science.

With these improved tables, Professor Airy has been enabled to settle the long contested epochs of several memorable ancient eclipses; as, for instance, those of Thales, and Agathocles, and that at Larissa; and has thereby established several important points in ancient chronology beyond all reasonable doubt. A curious fact connected with one of these eclipses shows in what manner the light of science reflected backward sometimes illustrates the dark points of history in the remote past. I am tempted to refer to it. It is stated by Diodorus

Siculus that the fleet of Agathocles, the tyrant of Sicily, was blockaded in the harbor of Syracuse, by the Carthaginians, with whom he was then at war. Shut up within narrow limits, and pressed on every side, he resolved on the bold and memorable project, then for the first time contemplated in the history of Carthaginian warfare, of "carrying the war into Africa." Under cover of the night, aided by many ingenious stratagems, he escaped from the harbor, and sailed for the African coast, pursued by the blockading fleet. Six days after his escape, having eluded the pursuit of the enemy, he landed at the "Quarries," burnt his ships, sacrificed to the gods, and commenced a campaign of conquest and slaughter which has few parallels in the history of warfare. An interesting account of it is given by Grote. On the day after the escape of Agathocles from Syracuse, "there was such an eclipse of the sun," says Diodorus, "that the day wholly put on the appearance of night, stars being seen everywhere." * History has left it uncertain when this memorable event occurred; and it has been made a critical question whether Agathocles passed round on the north or on the south side of the island. Singular as it may seem, the astronomical researches of Hansen and Airy have shown with more than historic certainty, that on the 15th of August, 309 before Christ, at six o'clock and forty minutes in the morning, Greenwich mean time, Agathocles witnessed a total eclipse of the sun, and must have been on the *south side of Sicily.*

But in the present advanced state of astronomical science, what, it may be asked, remains for astronomers to do? What new and noble achievements invite their toil? It would not be easy, perhaps not possible, to enumerate all the unsolved problems of astronomy. Several subjects of great interest may, however, be mentioned, which await further investigation.

(1) The true dimensions of the solar system, however much has been done in that direction, are by no means settled

* As quoted by Prof. Airy, Phil. Trans. Vol. LXXI. p. 187.

with that exactness which precludes the desire of closer approximation. The great primary problem, next to the size of the earth, is the distance of the sun. This has been pronounced by Airy to be the grandest problem in astronomy. I will not here detail the methods by which any error in the present determination may be eliminated or reduced. I shall refer to it again. With any correction in the sun's distance, all the planetary distances will take new value; and with these corrected distances, it may be necessary to reconstruct the tables of all the older planets. Then there are the asteroids, now numbering, I think, fifty-five (55). A vast amount of labor is necessary to determine their exact orbits. Their mutual perturbations will demand and receive attention. Nor will the possibility of their ever having found a single body, broken in pieces by some violent shock of nature, be permitted to pass without severe scrutiny; nor the other possibility of two or more of them being, by their mutual attractions, drawn together and forming a double planet. The physical constitution of the planetary bodies, and especially that of the sun, presents questions of great interest, which it would be unphilosophical to despair of being able to solve. The solar spots, of which the number has been very large during the present season, deserve, it would seem, to be scrutinized with the highest powers of the telescope. The present theories respecting them are, by no means, so satisfactory as to preclude further and more thorough investigation. Such an inquiry would probably determine more exactly than is now known, the time of the sun's rotation on his own axis. Whether it would throw any light upon the cause of their greatest and least abundance in periods of about eleven years, as discovered by Schwabè, time must determine. Do these spots occasion any sensible change in the solar radiation, and consequent change in the temperature and productions of the earth? Do they exist equally in all parts of the equatorial region? Or are there special localities for the development of those immense tornadoes whose ravages we see in the blackened face of the sun?

(2) Leaving our own planetary system, when, let us ask, will the department of cometary astronomy be exhausted? At any moment, these strange visitors may burst upon us in mild or terrific splendor, as the case may chance to be. What variety of form do they present, what tenuity and what rapid and prodigious diffusion of luminous matter, driven off as if by the impulse of solar light? How soon the earth will come into collision with some one or more of the millions which are supposed to exist, and they swallow us, or we swallow them, none can tell. It requires evidently a special corps of observers, a scientific vanguard, to watch their movements and ward off. But, gravely, what are the internal forces which produce such immense diffusion of cometary matter in so short a time? Do the portions of the tail driven off many millions of miles from the nucleus, ever return to it? Or do they remain suspended in mid ether, in a condition to be gathered up by the next one which sweeps along the sky?

(3) Sidereal astronomy is itself a subject of boundless extent. With the aid of instruments of great refinement, many thousands of stars have been observed and their places determined with great precision. But this is not, like the computation of a table of logarithms, a work for all time. At no very distant epochs the entire sidereal heavens must be reëxamined. The stars are said to be *fixed*, but reëxamination shows that very many of them are *not* fixed. They have motions of their own — some greater, some less. What may be the result of the comparison of places, accurately determined for very distant epochs, we cannot predict. If the catalogue of Hipparchus, transmitted to us by Ptolemy, containing upwards of a thousand stars, could have been based upon the precise determination of modern instruments, we might now, looking upon the exact changes of two thousand years, be able to predict the further changes for many thousands of years to come. We might have under our immediate inspection, as in the case of the planet Neptune, a small portion of the orbit, which would enable us to describe the whole. In that case,

to the contemplation of the astronomer, how grand and solemn would now be the order and movement of the starry hosts! To temporary inspection they are all fixed; — to the eye of a thousand years *all are in motion*, — all pursuing their appointed orbits to be circled only in millions of years.

(4) How much is yet to be known of the three thousand and upward of double stars already catalogued! The periodic times, the positions and forms of the orbits of several, have already been proximately determined. In one instance, at least, that of Gamma Virginis, we have seen one star occulted by the other. It is said that in 1836 no telescope in Europe could separate the two stars. They are now quite wide apart. Continued attention to this class of objects will undoubtedly reward the inquiry with most interesting results.

(5) But what shall we say of variable and periodic, of new and lost stars? What a boundless field of observation, inquiry, and conjecture, does this subject open before us! — a field scarcely yet entered upon, but one which is, no doubt, destined to yield the richest harvest to the labors of future astronomers. What are the physical conditions which determine Algol, in the short space of three and a half hours, to change from a star of the second, to one of the fourth, magnitude, remain at its minimum brightness fifteen minutes, and then in three and a half hours more return to its former magnitude, and go through the same changes continuously in a period of little more than two days? What shall we think of Mira Ceti, which remains completely invisible to the naked eye for five months, then increases for three months, when it attains to the brightness of a star of the second magnitude; retaining this brightness for about two weeks, it again decreases for three months and disappears as before? It occupies about eleven months in passing through these changes. A still greater wonder is Eta Argus, which changes at irregular intervals from a star of the fourth, to one of the first magnitude, second only to Sirius in brightness. The whole number of variable stars which have

been subjected to a more or less careful examination, is from thirty to forty. Hind has remarked that the prevailing color among them is of a *reddish hue;* and in several instances, at least, during the period of least brightness they are surrounded by a hazy or nebulous light. These facts seem to point to some common characteristic. They indicate like physical conditions. "The whole subject of variable stars," says Herschel, "is a branch of practical astronomy which has been too little followed up, — it holds out a sure promise of rich discovery." It is also a branch in which amateur astronomers, with moderate instrumental means, or even with good eyes alone, can labor to advantage.

(6.) But we have not yet reached the widest field of inquiry. With telescopes of good definition and great power of illumination, the nebulæ present objects of interest which are hardly second to any others. The results furnished by the great reflector of Lord Rosse encourage the hope of most decided progress in this branch of astronomy. Whether the nebulæ are all groups of stars capable of being resolved, or in some cases immense diffusion of luminous matter in the slow process of condensation, — worlds undergoing a formative process, — they are alike wonderful to contemplate.

Such is a mere glance at the unexplored regions of Astronomy.

It is now time to ask, What may American astronomers be justly expected to do in advancing this, the noblest of sciences? We may best answer this question by looking at what they have done. In adverting to this topic it will be my endeavor not to transcend the limits of modesty and truth. I think it will appear from the educational and scientific history of the country, that scientific investigations, and especially astronomical pursuits, have always been held in high estimation among us. In proof of this I may allude to observations made at the last two transits of Venus. It is well known that Prof. Winthrop of Harvard College, visited Newfoundland in 1761, to observe the transit which occurred on

the 5th of June of that year. It is not, perhaps, so well known that the "Province Sloop" was fitted out at the public expense to convey himself and his party to the place of observation. Elaborate preparations were made in different parts of the country to observe the transit of June 3d, 1769. Private munificence in the purchase of suitable instruments, and the coöperation of public bodies, bear honorable testimony to the enlightened zeal which animated the friends of science at that time. The observations of Rittenhouse and his associates, in Philadelphia, of Winthrop in Cambridge, and West in Providence, rendered important aid in determining the problem of the sun's parallax.

But while an honorable appreciation of astronomical science has been perpetuated among us, strange as it may seem, until within a very recent period, we have wanted the instrumental means and appliances necessary to prosecute it on independent grounds. So recently as 1840 we had done almost nothing in the way of astronomical experiments. There were at that time in possession of the coast survey, several fine instruments for the determination of latitude, for transits and for moon-culminations; and the reports of that department show how well they were used. Yale College had an altitude and azimuth refractor, by Dolland, of ten feet focal length, and five inch object glass. This at the time of its reception in 1830 was much the largest refractor in the country. Its mounting in the tower of one of the college edifices was by no means equal to its optical power. Williams College had a Herschelian reflector of ten feet focal length, equatorially mounted and a very good transit instrument. Hudson Observatory, in Ohio, had an equatorial refractor of four inches aperture. The fine telescope of the Philadelphia Observatory — an equatorial of eight feet focal length and six inch object glass, by Merz and Mahler, of Munich — was received in the autumn of 1840. Its erection marks an era in the history of our astronomical instruments. In optical power, and in the fineness of its graduations, it was quite superior to any thing which we

then had. It was, moreover, the first of the fine German telescopes imported into the country. ' When I have mentioned these instruments I have given a summary of our means of astronomical observation at the close of 1840. We had many other telescopes, that were useful in observing eclipses, occultations, and transits, and which afforded interesting views of many of the heavenly bodies, but none with high optical powers and accurate graduations, for the exact determination of the elements of position.

What have we done in this department within the last twenty years? I will not occupy your time with a history of American observatories, which must be familiar to the most of you. Suffice it to say, that we have now several not unworthy of comparison with some of the old and world-renowned observatories of Europe. Astronomical instruments have two objects in view; the one is the increase of optical power, the other is the exact determination of position. The one class of instruments scrutinizes the celestial bodies, and shows us *what* they are, the other tells us *where* they are.

As it respects optical power, we have at least six telescopes in use, which are entitled to high rank. The first in size is the great Munich Refractor at Harvard University, of twenty-two and a half feet focal length and fifteen inch object glass, made after the same models as the great refractor of the Russian observatory at Pulkova. These two instruments stand alone. Among refractors they have no equals. The results given by the Cambridge instrument in researches upon the nebulæ, upon the ring of Saturn, in the discovery of the eighth satellite of that planet, and upon the nucleus of the brilliant comet of 1858, fully attest its power. The Munich telescope at Ann Arbor, has an object glass of thirteen inches aperture. The telescope at the observatory of Hamilton College, in the State of New York, made by Spencer, has an object glass of thirteen and a half inches, and one at the private observatory of Mr. Rutherford, in the city of New York, made by Fitz, has also an object glass of thirteen and a half inches. These

three instruments, two of them of American workmanship, are reputed to be of excellent quality. They are all equatorially mounted. The refractor of the Cincinnati Observatory, with a twelve inch object glass, and that of Washington Observatory, with an object glass of nine and six tenths inches aperture, have both rendered good service to astronomy, and have acquired a high reputation for excellence. They are both from Munich. The instrument at Washington is of the same size as that which was used by Struvè for many years, with such signal success, at Dorpat.

For the coördinates of position, the right ascension and declination, we have mounted and in use, six meridian circles of the most refined construction, reading off to a single second of arc, and with the microscopes attached, descending practically to tenths of seconds, while the chronometers give the time to intervals much less than the tenths of seconds. These circles are at the observatories of Cambridge, Washington, Georgetown, West Point, Ann Arbor, and Albany. The last-named instrument, constructed under the special supervision of the late director of the Dudley Observatory, Dr. B. A. Gould, contains improvements which, it is believed, place it second to no other of the kind which has ever been made. It will remain a proud monument of the ability and practical skill of the director. It is understood that the other instrumental equipments of this observatory will be of the highest order. And it is hoped that an institution so nobly conceived and so auspiciously commenced, will, notwithstanding the adversity which has unhappily befallen it, and checked its progress, yet fulfil the distinguished promise of its inauguration.

In addition to these, there are in the possession of collegiate institutions and in the hands of amateur astronomers, I think, not less than twenty instruments, which are capable, under favorable circumstances, of showing the principal division in the ring of Saturn, and of opening double stars from 1″ to 2″ apart. Many of these are equatorially mounted,

and from time to time will be able to furnish most valuable contributions to astronomy.

It will thus be seen, that with our public appreciation of science, and the princely munificence of private citizens, we have in the short space of twenty years furnished ourselves with the means of embarking with advantage upon the most refined and difficult branches of practical astronomy. In addition to this, we may say, without undue self-esteem, that we have many observers among us who have attained to great practical skill in observation; observers who have studied the theory of errors in construction and adjustment and observation, and have made themselves proficients in the best method of detecting and eliminating them. The published proceedings of this Association bear honorable testimony to the profound ability with which the most difficult problems of celestial mechanics have been discussed before us. The volumes of the Washington and the Cambridge Observatories speak for themselves. They show that American observatories may be relied upon for the most accurate determinations. The astronomical expedition to Chili, which had for its object a new and independent determination of the solar parallax, reflects great credit upon the scientific enterprise and ability of Lieut. Gillis. The elaborate computations made by Dr. Gould, give for that important element a result extremely near to the received value as based upon the transit of Venus in 1769. And, what is pertinent to our present purpose, the new determination (8″.5) is based alone upon American observations.

It is understood that the observatories of Cincinnati and West Point contain records of high value which have not been made public. The astronomical and geodetic labors of the United States Coast Survey, including the method of longitudes by the electric telegraph, show consummate skill in the conduct of difficult researches, and reflect the highest honor upon the scientific character of our country. We think it may be said with safety, that none of the great geodetic enterprises of modern times, beginning with Picard's, has been

more thoroughly accurate than our own. The application of the electric telegraph to transit observations, known among the astronomers of Europe as "The American Method" — because conceived and perfected by American genius — is undoubtedly one of the greatest improvements in practical astronomy known to the history of the science. It enables the observer to do in a given time quadruple the work which was possible without it, and with nearly quadruple accuracy. The American Nautical Almanac, representing numerically the latest and highest improvements in planetary astronomy, shows what progress we have made in the difficult art of computation. I call it a difficult art because in a work of five or six hundred pages of closely printed figures, the intention and the expectation is that it shall be free from errors. To achieve this is a work of the utmost difficulty. It is a record of the ability of American computers, and in some respects a record of American science. I may extend this last remark to the Astronomical Journal, edited by Dr. Gould, which, in the main, presents American astronomical science in its most refined, practical, and analytic forms.

In the manufacture of the requisite qualities of glass for telescopes and in the construction of instruments of the very largest descriptions, we have not yet attained to the excellence of our older neighbors on the other side of the Atlantic. But who shall say that even that is a forbidden field to the genius or American artisans? Who shall say that Guinard and Frauenhofer will not find their proud rivals in American workshops to share with them the glory of perfecting the noblest of instruments, the astronomical telescope? The eminent success of Spencer, Fitz, and Clark, in telescopes of large size, to say the least, gives us substantial ground to hope that with due encouragement on the part of the public, we shall erelong in this department be able to meet the severest demands of advancing science.

There is still another and higher department of astronomy which, in some directions, forms the solitary basis of further

progress. Without it, facts may be observed and accumulated, but the remote and hidden laws of which they are consequences and ultimate exponents, can never be reached. I refer to that higher mathematical analysis which seizes the actual conditions of the system and demonstrates from them the laws which must exist, or which takes the laws as we now have them and demonstrates what must be the condition of the system at any time to come. A higher and nobler function of the human mind than this we can hardly conceive, below that of paying its devout homage to the Supreme Architect of these countless worlds above us. And here the name of Bowditch stands as a proud monument of American science. The profoundest problems of the Mecanique Celeste did not evade his searching scrutiny. But the name of Bowditch stands not alone. That of the lamented Walker is not unworthily associated with it. Other names there are, which delicacy forbids me to mention in this presence. Posterity, with even-handed justice and pious care, will assign them an honorable place among the great geometers of the age.

I have before alluded to the problem of the sun's distance. It is well known that the determination of that important element rests almost exclusively upon the observations on the transit of Venus in 1769. It is also known that the reliable observations on that occasion were fewer than could have been desired, and, strange to say, some which were given to the world were of doubtful authenticity. And we may add that however valuable the results obtained from observations on Mars in opposition, and Venus in conjunction, the next transit of Venus in 1874 is regarded as the surest means of verifying or correcting the previous determination of the sun's parallax. The astronomer royal of England has already called the attention of astronomers to this problem, and says that it is not too soon to begin the preparation for so important an event. He looks to the astronomers of the United States to take a leading part in the enterprise, and adverts to the peculiar advantage which will result from connecting

together a series of stations covering a great extent of country by that wonderful auxiliary to science, the electric telegraph.

To all the foregoing, I may add that the application of the art of photography to astronomy, now regarded as another "wonderful auxiliary," is due to American genius. The late director of the Cambridge Observatory, the lamented Bond, was the first to make the application, and show its practical importance. That sun-painting art, which delineates so quickly and so truthfully the features of the "human face divine," delineates with equal facility and equal precision the features of the sun, moon, and planets. The relative position of double stars and groups of stars is given with an accuracy scarcely inferior to the best micrometric measurements.

From the facts which have now been stated, the question again recurs, *What may justly be expected from American astronomers in advancing their chosen science?* But the facts themselves emphatically answer the question;—and that answer is, *Much every way.*

The material equipments are at hand, or if not at hand, will soon be furnished when solicited by the spirit of devotion and earnest progress. True, we have not the princely and governmental patronage which gives vitality and strength to many scientific establishments of Europe. But we have what few other countries can boast to the same extent, an enlightened public sentiment, a just appreciation of the dignity and importance of science, penetrating every portion of our community, and commanding not only the wealth of merchant princes, but the wages of hardy industry, to achieve the object of scientific culture. With a dependence like this, which is elastic, buoyant, versatile, and which readily adapts itself to the exigencies of pressure and demand, we need not shrink from the noblest enterprise. We have skilful observers, we have ready and practised computers, we have mathematicians who will be responsible for any contributions which analysis can make to the common progress. Such, gentlemen, is our position; such is the responsibility which rests upon us.

It remains for us to aid and encourage each other and show what may be done by a generous devotion to a noble cause. It may be that some of us will have occasion to profit by the precept of the Hebrew preacher, "If the iron be blunt and he do not whet the edge, then must he put to more strength." With the strength of firm resolve and the whetted edge of diligence, we may enter with good hopes upon the task before us. Our united efforts will surely not be fruitless.

# REPORT

ON THE

# HISTORY AND PROGRESS

OF THE

# AMERICAN COAST SURVEY.

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## PART I.—OBJECTS, HISTORY, METHODS.

### CHARACTER AND PRIMARY OBJECTS OF A GEODETIC AND HYDROGRAPHIC SURVEY.

To an industrious and enterprising people, zealously engaged in cultivating the arts of civilized life, and occupying a territory bordered to any considerable extent by the ocean, a correct acquaintance with the physical features which mark their marine boundary is a matter of such moment, that we may properly regard it as a kind of economical necessity. To such a people, the ocean is the great outlet of production, and the highway over which are perpetually flowing back upon them the products of other lands. Without an exchange of commodities constantly in this manner going forward between different and distant nations, it is impossible that the industry

of any people should be stimulated to an efficiency in any degree commensurate with its capacities: it is impossible that its inventive genius should be awakened, or its artistic skill developed, or its dormant natural resources evoked from their slumber, or, in a word, its powers of production generally brought into exercise in so effectual a manner as to promote most successfully and satisfactorily its advancement in the road to national wealth, or its elevation in the scale of national greatness. An isolated nation, with no natural channel for the efflux of its surplus production, will have little incitement to produce after the demand of its own consumption shall have been satisfied. The arts, moreover, among such a people, unfostered by the encouragement of a vigorous demand, will make but sluggish progress in the march of improvement, and will contribute but feebly to the multiplication of the comforts or the elegancies of life, to the growth of æsthetic culture, or to the increase of the general wealth.

It is true that in a territory wide of extent, and possessed of diversified resources, industry may find a stimulus in the upspringing of a domestic commerce. But it is not true that any activity in the exchange of productions at home can diminish the importance of commerce with foreign nations, or render it a matter of trivial moment to any people whether or not it shall avail itself of the natural advantages it may possess to foster such a commerce. It is accordingly matter of history, that every maritime people that has ever, since the days of the Tyrians, made itself conspicuous among the nations for its intelligence or enterprise, or has succeeded in securing to itself a controlling position in the affairs of the human race, political or social, has ever likewise found in foreign commerce one of the most prolific sources of its prosperity, and has been accustomed to keep a vast amount of its wealth constantly afloat upon the waters, and exposed to all the hazards which brood in the bosom of the deep.

These hazards can never be totally annihilated. They will always be the occasion of a greater or less amount of annual

loss. And maritime commerce, considered as one of the springs of national wealth, will always be by so much the less productive as the dangers to which it is exposed, and the losses to which it it consequently subject, are greater. Any expedients, therefore, by which these dangers and these losses may be diminished; any improvements, for example, in the construction of vessels, or in the art of navigation; or any additions to human knowledge in regard to the laws which govern the winds or the waves in their seeming caprices, or in regard to the secret dangers which lurk invisibly in the ocean's depths, will inevitably in due time be felt, in the increased returns with which they will reward commercial enterprise, and in the stimulus which they will bring to the growth of commerce itself. For it is to be observed, that, in the diminution or annihilation of the hazards which attend any speculative adventure of capital, a nation is profited not merely through the immunity from loss of each individual adventurer, but, beyond that, through the encouragement held out to other capital, unemployed, or employed unprofitably, to embark in a productive channel.

The dangers which attend the mariner are various. Some of them accompany him everywhere, and are capable of being reduced or eliminated only by perfecting the science of navigation itself, by improving the methods of naval architecture, and by insuring to practical seamen the highest nautical skill, by providing for their thorough training to their duties. Others, however, are local, and arise not out of the want of skill or science in the navigator, or out of the fragility or ill-construction of his vessel, but simply out of his unacquaintance with the natural features of the sea-bottom beneath him; with the currents of the surface on which he floats; with the degree of proximity or distance of reefs, or shoals, or dangerous shore-lines, or with the courses which he should follow in order to escape so perilous a neighborhood. Such local dangers are for the most part found in the vicinity of the land. Upon the high seas, indeed, the practised navigator dismisses apprehension; and though, as he leaves his port, he may lay his course with a severely

scrupulous accuracy, and trace his way by buoy or beacon, or feel it out with an almost timid caution with the lead, yet no sooner does he see the blue water rolling beneath him, than he experiences a sense of security as complete as that which the landsman feels in the shelter of his quiet home. As he approaches the port of his destination, his anxieties return, and his vigilance is once more aroused. At this critical period, ignorance or error may bring the most prosperous voyage to a sudden and fatal termination. The shores which mark the approaches to the most frequented seaports of our continent, have been a hundred times strewn with the wreck of vessels dashed in pieces just as the ill-fated voyagers whom they bore were congratulating themselves that all their dangers were over. The wealth which has been engulfed, almost within sight of a single American port, can only be told by millions; and the lives which have been swallowed up along with it present a melancholy total of many thousands.

Against the recurrence of disasters so calamitous, no absolute security can ever be provided. But it is obvious that their liability to occur may be very greatly reduced, by removing, so far as human effort can remove, the principal cause of their occurrence, which is simply want of correct knowledge of the natural features of the coast, as they appear exposed to view, and as they lie concealed beneath the waters. Accordingly the governments of all maritime nations have earnestly endeavored, especially in modern times, almost without regard to the question of expense, to secure a faithful and minute delineation and description of their entire ocean boundary, both as to its visible outline and as to the character of the soundings which lead from it into deep water. For the purpose of obtaining this information in the form most severely exact, systematic surveys, geodetic and hydrographic, have been instituted and sustained for many years, by the principal commercial powers of Europe; the results of their operations having been condensed, or being still in process of condensation, into the form of elaborate hydrographic maps, or charts, for the

guidance of the navigator, and also for the assistance of the government itself, in selecting proper sites for the lighthouses, buoys or beacons, which are to mark the available channels, or to warn against hidden dangers. Such a survey of our own coast, carried on under the authority of the Federal Government, has been also for a number of years in progress; and it is in regard to the past history and present condition of this great public work that the present committee has been appointed to inquire and report.

It is apparent, from what has been said, that the leading object directly proposed in the conduct of the American survey, as in that of every other, is to secure the safety of the wealth hazarded in commercial enterprises, by reducing to a minimum the dangers of navigation. But it may by no means hence be justly inferred that the benefits of such a survey are limited to peculiar localities, or confined to particular classes or portions of the people; that they accrue, for example, exclusively to the great marts of commerce, or to the individuals directly and personally concerned in foreign trade. To any one who attentively considers the causes which give to property its value, it will be manifest that these benefits extend to every citizen, no matter in what part of the country he may reside, provided that he has any thing whatever at stake in the prosperity of the commonwealth, even though it be no more than the labor of his own hands. The cotton-grower of Tennessee, and the tobacco-planter of Kentucky, whose agricultural products seek a market in the old world, are no less positively interested in whatever may diminish the dangers of the seas than the merchant who, without producing himself, makes it his business to convert their productions for them into the equivalent values they require. This remark is one of great importance. Nothing in these days is more common than to hear the three great departments of human industry — agriculture, manufactures, and commerce — spoken of as if they were interests entirely independent of each other, or even absolutely antagonistic. Yet, without the productions of

agricultural and manufacturing industry, commerce would have no material; and without the constant, and convenient, and rapid exchanges which commerce effects, agriculture and manufactures could have no life. Whatever makes commerce easy, or diminishes the cost or the hazard of its operations, encourages productive industry in every form, and renders its rewards more certain and more abundant. For the sake of illustration, let for one moment, the effect, so often illustrated under the eyes of the present generation, be considered, of opening up to public use a new line of communication, by means of a railroad or canal. There are thousands of farmers in this country, who, immediately on the occurrence of such an event, have seen the products of their labor practically doubled or tripled in value, through the consequent reduction in cost, trouble and loss of time, experienced in communicating with markets. There are exhaustless deposits of mineral wealth in every quarter of the country, which from being absolutely valueless, because unavailable, have become sources of rich income to their owners, simply in consequence of the creation of such artificial channels of transportation. Now, as the aliment of commerce is the surplus production of the country, it is self-evident that, if the ocean were to become suddenly impassable, this surplus production would be to the nation precisely what the unsalable produce of the isolated inland farm, and the slumbering wealth of the inaccessible mineral deposits, are to their individual owners. Since it could find no efflux, it could bring back no return. And though, were such a state of things suddenly to supervene upon the arrangements of business as they actually exist, the first ruin would fall, of course, upon the commercial holders of this surplus production; yet it requires no argument to prove that the permanent suffering must fall ultimately upon those who created this surplus — must fall, in short, upon the whole body of producers in the country; and this, too, without any regard to the question whether or not each has contributed his distinct share to the visible surplus, or whether only his industry has been em-

ployed and rewarded by those who have actually done so. And any conclusion at which we may legitimately arrive, in regard to the effect upon the national welfare, of an entire extinction of foreign commerce, arbitrarily supposed to occur, is measurably true also on the supposition that the same commerce is obstructed by impediments which embarrass its operations, but fall short of its total extinction.

Such impediments do actually exist in the natural dangers of the ocean border. The sea-coast lies outstretched, a continuous, unbroken barrier, obstructing every avenue through which it is possible for the surplus production of the country to find its efflux or receive its returns; laying the whole industry of the nation, in every department, and throughout the entire extent of its territory — in the pioneer settlements of the West no less than in the busiest of the commercial *entrepôts* of the Atlantic seaboard — under a perpetual and inexorable tax. To effect a reduction of this tax is the present business, as it was the original design, of the Coast Survey; and the interests of the States which lie most deeply situated in the heart of the continent are not less positively involved in the careful execution, and in the prosecution to completion of this great work, than those of any other.

To such as have never attentively reflected upon this important subject, it may not readily occur in how great a variety of modes the knowledge gathered in a properly executed survey of the coast is instrumental in preserving the seaman from the disasters to which he is liable. There is no difficulty, indeed, in understanding how, if the practicable channels are traced out and marked by buoys, and if the natural landmarks visible at a distance are accurately mapped down, with their bearings as observed from point to point, and if artificial beacons are erected on the headlands, or on the reefs or shoals, a navigator with the chart before him, may be able, in clear weather and by day, to pick his way in safety by the eye alone. There is no difficulty in understanding how he may, even on clear nights, be sufficiently secure in depending upon the guidance

of the beacon-lights of which the survey has fixed the most advantageous localities. But it is not in the broad light of day, nor yet when the heavens are serene by night, and the landmarks and beacons are easily discernible, that ocean disasters occur most frequently, or take the navigator most usually by surprise. The moments by far the most perilous, are those in which the solitary voyager is, as it were, walled out from the world by an impenetrable barrier of mist, limiting the range of his vision, in every direction, to a distance often not greater than his own vessel's length.

In such a situation of perplexity, helpless as it may appear, the charts of a perfect coast survey furnish a guaranty of safety, which, though it exacts of the mariner a more untiring vigilance, is yet no less to be relied on than the guidance of visible beacons. The cast of the lead informs him of the depth of water beneath him, and of the nature of the materials which form the bottom encountered by the plummet. A few successive casts indicate to him the degree of inclination, and the direction of the slope. With these elements to guide his judgment, he finds his place upon the chart without difficulty, and traces the line of his progress step by step. And it is the description of information here indicated which is most valuable of all to sailors, since this is the only species which is available when they are in their greatest peril. Yet it is obvious, without argument, that this is a kind of information which can only be perfectly gathered by patient, persevering, and long-continued labor; and which, in consequence of the changes continually going forward in the bed of the sea, can only be preserved in its accuracy and its value, by frequent and careful revision.

Another mode in which a scrutinizing examination of the coast contributes to the safety of navigation is, by the discovery of sheltered bays or inlets to which vessels may resort in stress of weather, and where they may lie in safety till the winds and waves recover their tranquillity. Such *harbors of refuge*, as they have been not unaptly called, have been found

to exist in many parts of our extended coast, having been quite unknown even to the sailors who frequent most the neighboring seas, until the searching operations of the Coast Survey had brought them to light. Commerce having failed, for whatever causes, to appropriate these harbors to its uses, whether on account of their inconvenient situation in reference to the interior of the country, or on account of the wildness or sterility of the surrounding territory, or simply for the reason that their capabilities had been overlooked; all the benefits which have flowed from their discovery, all the lives which they have rescued from destruction, and all the wealth which they have snatched from the jaws of the deep, are undeniably to be passed, wholly and entirely, to the credit of this great public enterprise. And were it possible to strike a balance between the amount of outlay, on the one hand, by which results of such value and importance have been accomplished, and the amount of material wealth on the other, which they have redeemed from the waves, it would unquestionably appear that the sum total of all the appropriations which have been made from the public treasury for the prosecution of the work from the beginning, has been amply more than overpaid by the benefits which have crowned some of these, its subordinate and least conspicuous achievements.

But though the leading object contemplated in a survey of the coast is to provide for the security of navigation, and thus to subserve the interests of commerce and of humanity, still there are other important ends incidentally, but at the same time necessarily, promoted, in the conduct of its operations, which are too important to be disregarded, and which must be taken into account, if we would comprehend the full measure of the benefits which such a work is capable of conferring upon mankind. These will be more particularly exhibited in the detailed account which will presently be given, of the results which have been already brought out, in the prosecution of our own Coast Survey. It is sufficient here to state in brief, that they embrace the accurate determination of favorable

bases for the institution of trigonometrical inland surveys, which may lead to the construction of scientifically exact maps of the territory of the several States; of observations on the phenomena of terrestrial magnetism, and the laws which govern their variations; of investigations of the difficult problems connected with the irregularities of the tides and of the winds; of the exploration of the Gulf Stream and other ocean currents; of the study of the distribution of submarine deposits by these currents, and the formation of such deposits near the points of discharge of the numerous rivers which intersect the coast; of the examination and settlement of questions relating to suspected or probable changes of the relative level of land and sea, and other points of the highest interest to geological science; of the determination of the sites most advantageous for the construction of national works of defence, and many other matters of economical or scientific interest, less general in their character.

## METHODS OF PRACTICE EMPLOYED IN THE CONDUCT OF A GEODETIC AND HYDROGRAPHIC SURVEY.

Such being the diversified and valuable ends, for the attainment of which so numerous and elaborate and extensive hydrographic surveys have been set on foot in modern times, along the coast lines of our own and of foreign countries, it is consistent with the design of this report to sketch in outline the methods of practice which have been employed in the actual execution of works so magnificent, so truly national in character, so incalculably useful, and so honorable to the governments which have undertaken them, and of whose wisdom they are among the most splendid and enduring monuments. The information here embodied would be unnecessary and out of place, were it to be addressed to geometers or physicists only: but if the committee rightly interpret the purpose of the Association in their appointment, it is a part of their duty to prepare such an account of the great public work referred to

them for review, as may be proper to lay before the reading public at large.

GEODESY. — With the simple processes involved in ordinary field surveys, every one may be presumed to be sufficiently familiar to enable him on a slight consideration to perceive their insufficiency to determine correctly any considerable portion of the earth's surface, or of any long line of inland or sea-coast boundary. And whoever has reached this point will find no great difficulty in understanding the general principles on which the peculiar methods adapted to such extensive works are founded. In an ordinary survey, the surface of the earth is regarded as a plane, and every field is considered to be a part of this plane, bounded by horizontal lines. Now as the earth is not plane upon its surface, but spherical, it is obvious that there is error even in the smallest survey executed according to the ordinary rules. And it is no less obvious that if two fields, side by side, be independently surveyed, the two planes supposed to represent them will not be mutually extensions of each other; but will be slightly inclined to each other, the angle of their inclination being equal to the amount of the earth's curvature between their middle points. In the case we have just supposed, indeed, this angle would be so very slight as to be altogether imperceptible: but should we compare two fields separated from each other by the distance of a hundred miles, we should find their inclination to be so great, that the plane of one of them extended in the direction of the other, instead of coinciding with it, as it would do if the earth's general surface were itself a plane, would pass above it at a height of a mile and a quarter.

Again, in defining the boundaries of fields, it is necessary to refer them to certain fixed lines of direction, certain cardinal points of the horizon, or, as they are commonly called, points of the compass. Now if the earth were a plane, all the lines which we call north and south would be parallel to each other; and so far as the limited space comprehended within a field or a farm is concerned, such lines are actually

sufficiently near to parallelism for every practical purpose. But since the earth is spherical, all such lines, wherever drawn, must intersect each other at last if extended to the poles; and they must consequently be more or less convergent in every other latitude. The extensive surveys of the public lands, therefore, executed by the government surveyors according to the ordinary methods, exhibit but a loose approximation to correctness; and they have only been preserved from intolerable inaccuracy, by dividing the territory to be surveyed into zones limited by parallels of latitude at moderate distances from each other; and resting each successive zone upon a new base. To illustrate, by specification, the magnitude of the error involved in this mode of surveying, when pursued over long lines or extended surfaces, it may be observed that such a survey carried from the shore of the Gulf of Mexico to the northern boundary of the State of Mississippi, would be erroneous to the extent of four miles in every hundred.

But in a survey designed to represent the features of a coast with such exactness as to assure safety to the navigator who depends on the information it affords him, there must be no question of miles or even of fractions of miles of error; the rigorous demand is, that there shall be no sensible error at all. And therefore it is that the methods adopted must recognize, in the first instance, the fact that the earth is a sphere. And all the lines and surfaces involved in the various geometrical and trigonometrical operations, must be reduced to conform to this fundamental requisition.

But even this does not represent the entire complication of the task. The results which would be obtained by the strict application of the processes of spherical geometry and trigonometry to the globe of the earth considered as perfect in figure, though immensely nearer to the truth than any that could be reached by plane trigonometry alone, would still fail to exhibit the points of the earth's surface as nature has presented them. For the earth after all is not a perfect sphere; but is larger in equatorial than in polar diameter by about twenty-

six miles. It is this deviation from the truly globular shape which makes of the earth what is called an oblate spheroid; and this must still be allowed for, before a survey like that which we are considering can possess the accuracy that either science requires, or the safety of navigation demands.

To a survey of this kind is applied the term, *geodetic*. Having indicated the nature of the geometrical principles upon which its determinations rest, it may be proper to give a brief account of the instrumental methods which are employed in the field in gathering the data for these determinations. The first step, which is to serve as the basis of all subsequent operations is, to measure with extreme accuracy, in a convenient and level spot, a base line of several miles in length. And when we speak of accuracy, it is to be observed that the word as here used is to be received in a much more severely rigorous sense than is the case in its ordinary applications. By the methods in practice in the American Coast Survey, a line ten miles long may be measured with no greater error than a fraction of an inch.

After this first very delicate but mainly mechanical process has been completed, the next thing to be done is to fix upon some prominently conspicuous and conveniently distant point, visible from both extremities of the base, and forming with them, by imaginary aerial lines, a triangle of large dimensions. All the angles of this triangle are then very carefully measured with instruments of the nicest accuracy; and from the values thus obtained and the known length of the measured base, the length of the unknown aerial sides is determined by trigonometrical computation.

Each of these unknown sides having become now known, may in its turn be made the base of a new aerial triangle; its extremities furnishing two of the angular points, and some new station still more distant, and falling conveniently in with the plan of the survey, supplying the third. Thus, by the continued repetition of the process here described, a chain of triangles is constructed step by step, which follows generally the

windings of the coast, but extends also as far inland as may be necessary to obtain for the triangles the dimensions most favorable to accuracy. For it need hardly be remarked that, if there be minute errors committed in the instrumental parts of the work, as is doubtless unavoidable even when instruments possess the extremest delicacy which art in its highest refinements has been able to secure, these, though singly imperceptible, may grow by repetition and accumulation, until error actually sensible begins to vitiate the results. On this account, therefore, it is desirable that the instrumental measurements should be as few as possible: and accordingly, in this principal chain of distances, the triangulation points are chosen as far from each other as the nature of the country will admit. It is a consequent necessity that the network of triangulation, in order that it may fulfil every important condition, should occupy a band of considerable breadth along the coast. Indeed some of the lines which form the sides of these aerial triangles, are not less than seventy miles in length; yet it is entirely demonstrable that the accumulated total of all the errors with which they are vitiated, does not exceed a single foot.

As a check upon errors of this description, and as a means of testing the precision with which the instrumental and numerical processes have been conducted along an extended line of triangulation, new bases, called *bases of verification*, are occasionally measured, often some hundreds of miles distant from the original base; and the length as trigonometrically computed through the whole chain, is compared with that which is found by actual measurement. So close has, in many instances, been the agreement between results obtained by methods so widely different as almost to exceed belief. A base of verification measured in France, in connection with the triangulation carried on through that kingdom for the determination of the length of a degree of the meridian, differed less than a foot from the value found for the same base, by computation through a chain of triangles, four hundred miles long.

A similar base upwards of seven miles in length, measured on Salisbury Plain, in England, showed less than a single inch of disagreement with the result deduced by calculation from an original base at the distance of one hundred miles. The American Survey furnishes an example no less remarkable. A base five and a half miles long on an island in Chesapeake Bay, as actually measured, and as determined through a triangulation of more than three hundred miles, presents results discordant to the extent of only four inches.

Next to the triangulation, in the logical order of proceeding, but practically simultaneously with it as far as convenience will allow, it becomes necessary to determine, with mathematical precision, the latitude and longitude of the principal points of the survey; and the angles which the lines composing the system form with the meridian. This is an essential pre-requisite to the subsequent projection of the whole in its proper place on the general map of the globular terrestrial surface, of which the several partial maps form a series of detached though successive portions.

TOPOGRAPHY. — The great primary triangulation having been established, the members of the system are afterwards subdivided into smaller triangles, called *secondary;* and these into others smaller still, called *tertiary.* By means of these, all the prominent points along the coast, and the principal features of its bays and harbors, are very accurately fixed. These form again points of departure, and checks to secure the accuracy of the final survey in detail, of the minute configuration of the coast-line, and of the characteristics of the land, embraced within the limits of the work; including its superficial irregularities, its lithological character, the vegetable growth which covers it, and all natural objects or artificial constructions which are in any manner noteworthy, or which affect its visible appearance. These operations are accomplished by a species of mathematical drawing conducted in the field, with the aid of an instrument called the Plane Table; and constitute the *topographical* portion of the survey.

HYDROGRAPHY. — All the laborious processes thus far described are confined to the land. Without them, the examination of the subaqueous irregularities, or the *hydrographic survey*, important as is this part of the work to mariners, could not be carried out so as to be of any substantial value. For it is the business of this branch of the survey to furnish an accurate delineation of the surface which the water conceals; and the positions of the remarkable points of this surface cannot be correctly placed on the chart, nor can the chart enable the navigator to recognize them, when he finds himself in their neighborhood, unless they are correctly determined in distance and direction from the objects visible on shore or above the water. The points, therefore, trigonometrically determined on the land, are used to fix the positions of sounding vessels, which are employed to ascertain the depth of the water and the nature of the bottom, — the positions selected being so much the less distant from each other as the irregularities detected are greater, the shoals more dangerous, the submarine deposits more varied, and the seas under examination more frequented.

Observations upon the tides and currents, on the variation of the compass, and on the effects of winds upon the level of the water, furnish the remainder of the data necessary to complete the information which is to be presented on the chart — necessary, in other words, to form a graphic representation, which shall exhibit with minute accuracy the latitude and longitude of every remarkable point of the shore-line, and of every promontory, hill, mountain, or other object which, when the land is in view, can aid the mariner in identifying his position: which shall show, also, the situations and windings of channels; indicate the localities of hidden rocks and shoals; give the depth of the water, and the character of the bottom, everywhere within the limits which can be reached in ordinary soundings; and, finally, furnish all the information in relation to tides and currents, light-houses, beacons, fog-bells, buoys and sailing lines, which is necessary to render navigation secure.

Such being the objects aimed at, and such the processes employed in conducting the difficult work of a coast survey, it seems proper, before proceeding to inquire how far these objects have been successfully pursued, or these processes skilfully and faithfully applied in the prosecution of the survey of our own coast, to review, in brief, the history of similar undertakings in other countries. To this topic, therefore, the committee propose to direct their next attention.

## ACTION OF OTHER GOVERNMENTS IN REGARD TO SUCH SURVEYS.

The national importance of works such as we are considering has for a long time been fully recognized by the governments of Europe. The French, who were the first to apply the principle of triangulation to the admeasurement of an arc of the meridian, with a view to determine the curvature and figure of the earth, were the first also to frame a territorial map upon the basis of geodetic surveys. The want of accuracy, however, in some of the elemental proceedings upon which this well-known map was founded, and the comparative meagreness of its details, gave rise to a scientific organization, inaugurated some fifty years ago under the auspices of La Place, for the purpose of its review and reconstruction. From that time to the present, the new geodetic survey of the kingdom, republic and empire has been steadily advancing; and, at the date of this report, about four fifths of the work, embracing maps of some one hundred and seventy thousand square miles, may be regarded as nearly completed. The hydrography of the coast is finished. The cost of these magnificent contributions to the political importance and domestic interests of France cannot be stated definitely; for the departments to which they have been confided have had other duties in charge; but the annual expenditure has been estimated at a little less than three hundred thousand dollars. The charts for general use are on a scale of rather more than three quarters of an inch to the mile; some of them, intended to direct

in the construction of public works, are on a scale four times larger.

The trigonometrical survey of England proper was begun in 1791, and will be finished in a few years more. A million of pounds sterling has been expended in its prosecution. The surveys have been made on a scale of two inches to the mile, with the exception of the six northern counties, where the scale was increased to six inches to the mile. The published charts are on a scale of one inch to the mile, and 108 sheets will cover the whole area of England and Wales.

The surveys of Scotland and Ireland are independent of that of England. They were begun later, and have advanced more rapidly; but they have been pursued into greater minuteness of detail, and have involved a greater rate of expenditure. In one of the documents laid before Parliament a few years ago (1851), the surveys of the three kingdoms were estimated to require, for completion, the sum of four millions one hundred and thirty-three thousand pounds sterling — a sum equal to more than twenty millions of dollars.

The Austrian government has been, for some eighteen years, engaged in a survey of its dominions, at an annual cost of half a million of dollars. The Russian, the Prussian, the Swedish and Norwegian, the Sardinian, the Belgian and the Spanish governments — indeed, all the governments of Europe, without a noticeable exception — have actively occupied themselves with the topography of their respective territories. A summary view of their labors, so far as definitely ascertainable at the latest dates, is exhibited in the following tabular arrangement:

| Territory surveyed. | Scale of Maps. | Sheets finished. | Sheets incomplete. |
|---|---|---|---|
| England, | 1: 63,310 | 100 | 8 |
| France, | 1: 80,000 | 172 | 77 |
| Belgium, | 1: 20,000 | completed. | |
| Netherlands, | 1: 50,000 | 12 | 50 |
| Hanover, | 1:100,000 | 62 | 0 |
| West Prussia, | 1: 60,000 | 59 | 0 |
| East Prussia, | 1:100,000 | 190 | 56 |
| Upper Prussia, | 1:150,000 | 23 | 0 |
| Hesse, | 1: 50,000 | 36 | 0 |
| Wurtemberg, | 1: 50,000 | 56 | 0 |
| Baden, | 1: 50,000 | 56 | 0 |
| Switzerland, | 1:100,000 | 10 | 6 |
| Bavaria, | 1: 50,000 | 90 | 6 |
| Tyrol, | 1:144,000 | 23 | 0 |
| Sardinia, | 1: 86,000 | 41 | 0 |
| Parma and Roman States, | 1: 86,000 | 8 | 0 |
| Illyria and Styria, | 1:144,000 | 37 | 0 |
| Austria, | 1:144,000 | 37 | 0 |
| Bohemia, | 1:144,000 | 23 | 14 |
| Saxony, | 1: 57,000 | 14 | 3 |
| Silesia, | 1:144,000 | 23 | 35 |
| Gallicia, | 1:150,000 | 23 | 0 |

Besides these, which, as the table shows, have produced more than eleven hundred sheets of finished map-work, there are the completed survey of Greece, and the surveys of Sweden, Denmark, Norway, Spain, and other countries, yet in progress.

In the course of the works, under the guidance of the ablest astronomers of their times, — La Place, Arago, Struve, Bessel, Schumacher, Gauss, Carlini, and their compeers, — the entire surface of Europe has been traversed, and a series of great arcs has been measured, extending together over nearly forty degrees. The chains of triangulation, which were constructed in the geodesy of each country, have been connected into one harmonious system; and it is not too much to say, that, in a very few years, every town and hamlet, and isolated villa — every mountain and hill, and lake and battle field — will have its place assigned it, with mathematical precision, upon the general map of the European continent.

The hydrography of the coasts has been the object of equal care. Admiral Beechy, in his address before the Royal Geographical Society, in 1856, speaking of the hydrographic sur-

veys of the north of Europe, which had been conducted by Russia, Prussia, Sweden and Denmark, says: "No fleet ever left England so well supplied with charts as the Baltic fleet of the last war."

The survey of the East India Company's possessions, under Colonel Lambton and Lieutenant-colonel Everest, is distinguished among the geodetic achievements of the time, no less by its magnitude than by the skill and energy with which it has been prosecuted. It has measured, by great triangulations, a meridian arc of twenty degrees and twenty-one minutes, or of about fifteen hundred miles in length. Up to the present time, it has delineated four hundred and forty-seven thousand square miles of territory, being about one third of the whole area of India.

The hydrographic operations of the Company have embraced a part of the eastern coast of Africa, the Red sea, the Persian gulf, the Arabian sea, the Bay of Bengal, and the China sea. The charts, one hundred and forty-five in number, include the trace of a series of *reconnaissances* of all the shores, from the most eastern cape of Africa to China.

It would be out of place, in this very general review, to pursue into greater detail the history of these numerous and extensive and interesting foreign works; or to enumerate specifically the various and important benefits which they have been the means of bestowing upon mankind. If the committee have succeeded in showing that the governments of the civilized world have generally favored public works of the kind under consideration, their object will have been attained. But they believe that they have done more than this. They believe that enough has been said to make it manifest that all enlightened governments have vied with each other in the liberality, the zeal, and the perseverance with which they have prosecuted their several surveys; and have displayed, in the spirit which has sustained them, a most laudable ambition to secure for their results the severest scientific accuracy.

## FIRST ORGANIZATION OF THE COAST SURVEY OF THE UNITED STATES.

It is believed that the honor of first suggesting a geodetic survey of the American coast, is due to the elder Professor Patterson, of Philadelphia;* who, as early as the year 1806, availed himself of his intimacy with the President, Mr. Jefferson, and the gentlemen who formed his cabinet, to impress them with the feasibility and policy of the measure. The act of Congress of 1807 was passed upon the executive recommendation. It authorized the President to cause a survey to be made of the coasts of the United States; in which were to be designated the islands, shoals, and places of anchorage within twenty leagues of the shore, with the courses and distances of capes and headlands, and such other matter as might be deemed proper for completing an accurate chart of every part of the coast. It also authorized the survey of St. George's bank, and any other banks or shoals, and the soundings and currents beyond the limits aforesaid to the Gulf-stream.

A circular was accordingly issued by Mr. Gallatin, the able statesman then at the head of the Treasury Department, setting forth a project of a survey, and inviting to it the attention of scientific men. In this project, the proposed operations were distributed under three heads: —

1. The ascertainment, by a series of astronomical operations, of the true positions of a few remarkable points on the coast.

2. A trigonometrical survey of the coast between these points.

3. A nautical survey of the shoals and soundings of the

* The above statement rests on the authority of Judge Kane. Since the first publication of this report, Gen. J. G. Swift has communicated extracts from his diary, from which it appears that Prof. Hassler moved in the matter as early as 1806; and there appears to be no doubt that he and Dr. Patterson united in urging the measure upon the attention of the government.

coast, of which the trigonometrical survey was to supply the basis.

Among the gentlemen who replied to the secretary's circular, was the late Mr. F. R. Hassler. This gentleman had, previously to this time, been engaged in the triangulation of the Swiss canton of Berne; and he proposed to place at the service of our government the ability and experience which he had acquired in the school of practice, for the execution of the American survey.

Mr. Hassler was accordingly appointed; and in the year 1811 he visited Europe for the purpose of obtaining the instruments necessary for his operations, all of which had to be specially constructed; but owing to the disturbed state of the continent, and the subsequent war between England and the United States, his efforts to obtain them were for a long time baffled. He did not return to the United States until the year 1816; and it was not until some time in the year following that a commencement of the work was at length made in the bay and harbor of New York. But the work had been hardly begun, before it was suspended in consequence of the failure of Congress to provide funds for its continuance; and in 1818 the law under which the superintendent had been appointed, was repealed.

From 1819 till 1832 attempts were from time to time made to survey portions of the coast under the direction of the Navy Department. Detached surveys of rivers and harbors were made; and hydrographic reconnaissances of the coast of some of the States; but no general or connected survey of the coast was attempted, nor did the detached surveys yield fruit which, taken as a whole, could be considered creditable to the navy or to the country. The Hon. S. L. Southard, Secretary of the Navy in 1828, in a reply to resolutions of inquiry from the Committee on Naval Affairs, in the House of Representatives, characterized the charts produced by those surveys as expensive and unsafe, pointed out the inefficiency of such a desultory plan of operations, and recommended a recurrence to the law of 1807.

On the repeated representations of Mr. Southard and others, the project of a geodetic survey of the coast conciliated again the favor of Congress in 1832. A small appropriation was then made for carrying out the law of 1807, and the President was authorized to employ in the conduct of the work, such astronomers and other persons as he should judge proper, in addition to officers in the land and naval service. From this time the operations of the Coast Survey passed anew under the charge of Mr. Hassler; and he continued to direct them till his death, which occurred in the year 1843.

In reviewing the history of this early period, it is proper to remember that "the first years were necessarily years of organization and instruction. The superintendent had to systematize methods, to train up assistants, to cause the work to grow from a small beginning, until it comprehended the various operations of a geodetic survey upon the land, and included the hydrography of the adjacent waters. When the results accumulated, it was necessary to provide for their computation and reduction; and also for the preparation of maps and charts upon a plan suited to our extended coast, and for the engraving of the maps themselves. All these things were new in this country. The amount of knowledge, skill, and labor required to overcome these and other difficulties was hardly appreciated. The results show how large an amount of work had been done, and how the work was extending beneficially at the time of Mr. Hassler's death."

The condition of the work as Mr. Hassler left it, will be made intelligible by the following brief statement. A base line had been measured on the south side of Long Island, in the vicinity of New York, the commercial importance of which obviously indicated it as the proper point of beginning. The triangulation had extended eastward to Rhode Island, and southward to the head of Chesapeake bay; the primary triangulation crossing the neck of New Jersey and Delaware, while a secondary triangulation skirted the coast of New Jersey, meeting and inosculating with another series which ex-

tended down Delaware bay. The topography had kept pace with the triangulation; and the hydrography of New York bay and harbor, of Long Island sound, of Delaware bay and river, and the off-shore work, from Montauk Point to the capes of the Delaware, were nearly completed.

A reference to the table of statistics in the second part of this report will show that the triangulation covered an area of 9,000 square miles, furnishing determinations of nearly 1,200 stations, for the delineation of 1,600 miles of shore-line; that 168 topographical maps had been surveyed, and 142 hydrographic charts. The work of publication had been organized, and five large charts were engraved, very nearly ready for publication.

The progress thus sketched, although really very considerable, and highly creditable to the late superintendent, was still felt to be inadequate to the pressing demands of commerce, and clamors arose in Congress against the administration of the survey, ascribing the slow progress to an unnecessary refinement in the processes employed, and claiming the results to be inadequate to the expenditure. An investigation was accordingly instituted in 1842, by a congressional committee, which, after a severe and unfriendly scrutiny, practically resulted in a complete indorsement of the principles on which the survey had been conducted by Mr. Hassler. A proviso was attached to the Appropriation Bill in 1843, directing that the survey be thereafter executed according to a plan of reorganization to be prepared by a board of officers, consisting of the (late) superintendent, his two principal assistants, the two naval officers in charge of the hydrographic parties, and four officers of the corps of topographical engineers.

The plan adopted by this board in 1843, and invested with the force of law by the preceding legislation, reaffirms the scientific methods proposed by Mr. Hassler as the basis of the work, and provides for the organization of its operations.

## EXPANSION AND MODIFICATIONS OF THE COAST SURVEY UNDER THE PRESENT SUPERINTENDENT.

In consequence of the death of Mr. Hassler, which occurred soon afterwards, the responsibility of amplifying and carrying into effect the provisions of the adopted plan devolved mainly upon his successor, the present superintendent, ALEXANDER DALLAS BACHE. The appointment of this gentleman was a concession to the universally expressed scientific judgment of the country. It is within the personal knowledge of some members of this committee, that our colleges, learned societies, and men of science, united in one strenuous call upon the executive, to withdraw Professor Bache from the field of more limited usefulness in which he was already distinguished, and to secure to the entire nation the benefit of his great talents and his services. And it is matter of just pride to them, that their elevated estimate of his merits has been so abundantly vindicated by the brilliancy of his official career.

Up to this time, it will be observed, the survey had been confined entirely to the Atlantic coast, and to the limited portion of that coast embraced between Narragansett bay and Cape Henlopen. No sooner had the new superintendent been able to form some just estimate of the magnitude of the work in hand, and of the great length of time which it must require for its completion, at its actual rate of progress, and with the existing provision for its prosecution, than he urgently recommended the adoption of a more comprehensive system, according to which the work should be commenced, and carried on independently, in many places at once; each section employing its own base, and making its own geographical determinations; but all designed to form, when completed, a single continuous and unbroken chain of triangulation extending from one end of the coast to the other. At that time Texas had not been annexed, and the western coast was not in the possession of the United States. The proposition of Pro-

fesssor Bache, reduced to a specific form, was to divide the entire coast of the Atlantic and the Gulf of Mexico, into eight distinct sections,* embracing as nearly as could be estimated, the same length of shore-line in each; and to commence the work in as many of these simultaneously, as Congress could be induced to provide for.

This recommendation was made at the close of the year 1844, and was immediately approved. Accordingly, in the course of the following year, the superintendent, besides extending the line of Mr. Hassler at both ends, commenced active operations on the coast of Virginia and North Carolina upon the Atlantic, and upon the coast of Alabama and Mississippi in the Gulf of Mexico. Two years later the work had been extended to the States of South Carolina, Georgia, Louisiana, and Texas; and two years later still the important survey of the reefs and keys of Florida was commenced.

On the annexation of California, the Pacific coast was immediately included in the plan of operations; and such was the vigor with which the work was pushed forward in every quarter, that Mr. Corwin, then Secretary of the Treasury, in a report communicated to the senate in 1851, was enabled to show, that a twofold increase of appropriations to the work had produced an increase in the amount of performance, and in the aggregate of the results obtained, more than threefold. And he traced this economy to the division of labor which an increased scale permits, and to the vigorous prosecution of the work at the South and at the North at the same time, whereby the same persons can avail themselves of the best season for operating in the field in each region. The secretary, at the same time, advised strongly against the reduction of the appropriations, or the diminution of the force employed on the survey; maintaining that such retrenchments could be made only at a large economical sacrifice.

* Since the acquisition of Texas and the extension of the work to the Pacific coast, the number of sections has been increased to eleven.

With this brief statement of the general administrative principles, according to which the present superintendent has aimed to conduct the operations of the important work confided to him, the committee proceed to consider, in more specific detail, some of the particular methods pursued in its execution, with special notice of those in which an advance has been made upon methods heretofore used in this or similar works, passing afterwards to a review of the results obtained up to the present time.

## DETAILS OF COAST SURVEY OPERATIONS UP TO 1858.

Under this head, the committee desire to acknowledge their obligations to the obliging assistants in the office of the Coast Survey at Washington, for information courteously rendered in regard to such matters of history and methods of practice as are not fully embraced in the published reports, and to which the personal knowledge of the members of the committee could not be presumed to extend.

MEASUREMENTS OF BASES. — There have been measured in all, up to the date of the present report, nine principal base-lines, two of which are now connected by a primary triangulation. These two are the Fire Island base, on the south side of Long Island, and the Kent Island base, in Chesapeake bay. Both of these, and also a third one in Massachusetts, were measured with the apparatus designed by Mr. Hassler, and fully described by him in the Transactions of the American Philosophical Society, Vol. IV.; the measurements being made with iron bars, and by optical contact. Four bars, each two metres in length, were clamped together endwise, forming a combined length of eight metres, and placed in a wooden box suitably stiffened, the ends of the bars projecting a little beyond the box. A powerful microscope, mounted on a suitable stand, was adjusted so as to bring its cross-wires to coincide with the forward extremity of the bars, which was defined by the dividing line formed by an abutting piece clamped against

it. The box was then carried forward and adjusted in position so as to bring the rear end under the same microscope; another was adjusted over the forward end, and the measurement continued in this way. The temperature of the bars was ascertained, as nearly as may be done by such means, by thermometers placed in contact with the bars. It will be observed that the correction for temperature is a very sensible quantity in a long line; supposing the temperature at which the standard has its determinate length to be 32° Fahr., as is the case with the metre, and the measurement to have been made at an average temperature of 72°, the nominal length of a line of 10 miles would be 4.5 metres, or nearly 15 feet less than the true length expressed in the standard measure. It is highly important, therefore, that the temperature of the bars during the measurement should be accurately ascertained, or else, that the expansion should be *compensated*, as in the case of a compensation-pendulum. A mercurial thermometer of ordinary form will follow changes of temperature far more quickly than an iron bar of some thickness, owing to the great *specific heat* of the latter; while the temperature is rising the thermometer will show a higher, and while it is falling a lower temperature than the iron bar will have acquired at the same time, and the measurement is liable to errors arising from this source, unless the errors in both directions are accidentally balanced in the aggregate.

To obviate this source of error, Professor Bache designed an apparatus, which was constructed in the office of the Coast Survey, under his direction, and with which the subsequent bases have been measured. In this apparatus the measuring bars are arranged upon the plan of compensation for the effects of temperature, which was first used by Colonel Colby in Great Britain, and afterwards in this country by Mr. Borden, in the Trigonometrical Survey of the State of Massachusetts; but the mode of obtaining the compensation is entirely different. A principle is also introduced, which, though of sufficiently obvious importance when once suggested, had not been

previously recognized; or had not, at least, been previously applied in practice. It is matter of observation that if a bar of brass and a bar of iron of equal dimensions be exposed to the same source of heat, they will not always undergo equal changes of temperature in equal time; but that, after a short interval, a difference will be found to exist, the amount of which will depend on the different conducting properties of the two metals, their different specific heats, and the different powers of absorption possessed by their surfaces. Accordingly, two such bars, if of equal cross-sections, will not form a system compensating during progressive changes of temperature, however perfectly the compensation may be ultimately effected, when the temperature shall have become stationary, and uniform throughout the system. In order, therefore, that a combination of such bars may answer the purpose aimed at, their sections must be so proportioned to their conducting power and specific heat, that their fluctuations of temperature under different exposures may go on at precisely the same rates. As to the absorbing power of their surfaces, that is easily made identical, by giving to both bars the same external coating.

The lever of contact and level, first used in the adjustment of standard measures by BESSEL, has been adapted to this apparatus, the contact between two sets of bars being made by a blunt knife-edge against a plane, both of agate. Those who desire to study this beautiful and delicate arrangement are referred to the Coast Survey Report for 1854, where it is described and figured. The mechanical arrangements by means of which the bars are adjusted in line, height, and inclination, are also worthy of admiration; and such is the rapidity with which the apparatus can be used, that on favorable ground a mile has repeatedly been measured in a single working day of ten hours. It is so arranged that the measurement may be made at inclinations up to 3° with the same facility and accuracy as on level ground.

The accuracy of its results has been practically tested by repeated measurements of the same distance, from which it ap-

pears that the probable error in measuring a mile is about two hundreths of an inch. This base apparatus was designed in 1845, and first used in 1847, on Dauphine Island, on the coast of Alabama. Since then base-lines have been measured with it in North and South Carolina, in Florida, and in Maine. All these measurements have been conducted by the superintendent in person, aided by some of his assistants. The average length of the primary base-lines measured thus far is about six miles.

Many laborious and delicate experiments were requisite to perfect the adjustments of the apparatus. Before and after the measurement of each base-line, the length of both sets of bars, which is six metres, or nearly twenty feet, is carefully compared with a standard bar of iron. The rate of expansion of this standard has been ascertained by experiment, and its length at 32° Fahr. has been verified by means of six single metres abutting endwise, each of which has been compared with an authentic standard metre — one of those originally made by the French committee on weights and measures. The possession of this valuable standard, which was brought to this country by Mr. Hassler, and by him presented to the American Philosophical Society, was one of the chief reasons for using the *metre* as the unit of length in the Coast Survey, especially in the absence of any legally established standard of the country. The length of the English yard, which is our customary unit of length, has but recently been definitively settled in Great Britain; and the fact that the dimensions of the earth have hitherto been best known in metres,* makes the latter measure one of great convenience in geodetic operations. The distances in the survey are, of course, also given in yards, and miles, for common use.

The comparisons of length are made by means of Saxton's *pyrometer*, an instrument of great simplicity, and affording the

* Or rather in terms of the *toise*, of which the metre is a certain aliquot part.

highest degree of accuracy. A small mirror, movable about a vertical axis, reflects into the field of a telescope, placed at a distance of 20 feet, the divisions of a long curved scale fixed directly under the telescope; by means of a small abutting piece attached to the mirror, any difference in length to be measured causes a corresponding rotation of the mirror, by which different scale-divisions appear on the wires in the telescope, and a greatly magnified indication is thus obtained by an index equal in length to twice the distance of the mirror from the scale. The quantity to be measured is thus readily magnified two thousand times, without any loss from inertia and flexure, which would be unavoidable in obtaining a similar result by means of mechanical levers.

The necessity for the greatest attainable accuracy in such operations is apparent when we remember, that an error in the base-line will affect the whole distance depending upon it by triangulation, in the same ratio: thus, if the base-line were in error by but its ten-thousandth part, a distance of one hundred miles depending on it would be in error, from that source alone, by one hundredth of a mile, or fifty-three feet.

The Fire Island base, on the south side of Long Island, and the Kent Island base, in Chesapeake bay, are connected by a primary triangulation. These are the bases alluded to in a former part of this report, where it is mentioned that the Kent Island base has served to verify the other. This Kent Island base is five miles and four tenths long, and the original Fire Island base is eight miles and seven tenths. The shortest distance between them is two hundred and eight miles; but the distance through the triangulation is three hundred and twenty. The number of intervening triangles is thirty-two; yet the computed and measured lengths of the Kent Island base exhibit a discrepancy no greater than four inches.

On some portions of the coast, where the pressing wants of commerce made it desirable to commence the work in advance of the regular primary triangulation, local surveys have been executed based on preliminary base-lines, which were measured

with wooden or iron bars, and some simple auxiliary apparatus, generally devised by the assistants having immediate charge of the work. Some of these contrivances have been described in the annual reports of the superintendent, and will be found very useful to persons wishing to execute local surveys with some degree of accuracy. See particularly C. S. Report, 1857. All such preliminary measurements will in time be superseded by measurements with the compensating apparatus above described.

MEASUREMENT OF ANGLES. — In measuring the angles of the primary triangulation, theodolites of various sizes and construction are employed. The largest instrument of this class belonging to the Coast Survey, has a circle of thirty inches in diameter, graduated to five minutes, and reading to single seconds of arc on three micrometer-microscopes; it is provided with a telescope of great power, which is partly supported by springs within the upright columns, so as to relieve the bearings in a great degree from friction. The weight of the whole upper or movable portion is in great part supported by friction rollers and taken off from the vertical axis, in order to secure the latter against wear and to obtain a very free motion about the axis. This instrument was designed by Mr. Hassler, and constructed by Troughton & Simms, of London. It is used by the superintendent in the execution of the primary triangulation of the north-eastern part of the coast, where the country presents considerable natural elevations and admits of long lines of sight, ranging from twenty to eighty miles in length. The accuracy of pointing and reading attained with this instrument is such, that, notwithstanding the varying condition of the atmosphere, the probable error of a single measure of an angle is about one second and a quarter, which in the mean of thirty such measures, the usual number taken, is reduced to about one-fourth of a second. The observations are made under all conditions of visibility of signals, varying from a well-defined and steady image, to a nearly obliterated, trembling blur. For each observation, the appearance of the

signal observed has been carefully noted; and the means have been thus obtained for a discussion of the results with reference to the conditions of vision. The results arrived at justify the conclusion that the mean of observations taken in various states of the atmosphere, is probably preferable to the mean of such as have been obtained under circumstances apparently the most favorable; there being in the former case less of correction required to fulfil the geometrical conditions of the work; while a given limit of probable error can generally be reached by that method, in much less time than by the other. This result may be readily accounted for, by supposing that, under the conditions which appear most favorable to accuracy of observation along a line of considerable extent, there may regularly occur a slight lateral deflection, which, in the mean of observations taken under different conditions of the atmosphere, is sensibly eliminated.

A theodolite nearly similar in construction to the one above described, but having only twenty-four inches diameter of circle, is likewise used in the primary triangulation. Besides these, *repeating theodolites* with circles varying from twelve to six inches are employed in the different classes of triangulation. On the Atlantic coast south of New York, and on the shores of the Gulf of Mexico, where the country presents no considerable eminences, and is in general densely wooded, the instruments frequently require to be elevated on wooden structures, in order to overcome the curvature of the earth, and to get above the most heated stratum of air. These structures, which range from twenty-five to fifty feet in height, have been constructed on various plans by the assistants of Professor Bache, and have proved amply steady for measurements with the repeating instruments.

The signals generally employed to mark station-points are straight poles, supported by a tripod, and surmounted by a cone or bell-shaped cap of bright tin, on which the reflection of the sun shows a luminous spot to the observer. On the long lines of the primary triangulation, however, *heliotropes*

are used in addition to the signal-poles. The heliotrope, invented by Gauss, is an instrument in which a mirror of a few square inches' surface is so mounted in connection with a telescope, that the reflection of the sun may be thrown to any desired direction. It is very effective, and it is not unusual to observe on heliotropes on the summit of mountains 80 or 90 miles distant, when the outlines of the mountains are invisible through the hazy atmosphere.

The number of observations taken for the different classes of triangulation is so apportioned to the quality of the instruments used, and to the agreement of the individual observations among themselves, that the error of the sum of the angles in a primary triangle will not exceed three seconds, or nine seconds in a triangle of the second order. These limits of error, however, are seldom reached in fact. The average correction required for an observed angle in the eastern section, where the stations are natural elevations and the sides average over forty miles in length, is about four tenths of a second. In the southern sections it is rather larger.

Observations of Latitude. — In order to fix the situation of the whole network of triangles on the surface of the earth, it would suffice, if the figure of the earth were precisely known, to determine the latitude and longitude of some one point, and the direction with reference to the true meridian of one of the lines — in other words, its azimuth. But owing to the irregularities of this figure, it is found necessary to repeat these determinations very frequently, by which means the work is checked, and any accumulation of error avoided, while at the same time the figure of that portion of the earth covered by the survey is accurately ascertained. Observations of latitude are made at numerous stations of the primary triangulation. Different methods and different instruments have been employed, and the results have been carefully discussed in order to determine which are best and most available. A two foot vertical circle, and several twelve-inch repeating reflecting circles, by Troughton & Simms; and also six-inch

and ten-inch repeating theodolites by Gambey, were tried successively, but the results were found to be liable to considerable instrumental errors, and the degree of their accuracy proved to be insufficient to repay the labor which had been bestowed upon them. Finding a larger class of instruments to be indispensable, Mr. Bache had recourse to the prime vertical transit, the zenith telescope or equal altitude instrument, and the zenith sector; each provided with a telescope of forty-five inches focal length.

The method of determining latitude by observations of transits of stars over the prime vertical, first used by Bessel, is admirable in theory, but in practice a great loss of time and labor is often occasioned by clouds, which interfere with obtaining corresponding sets of eastern and western transits; and for operations in the field this method is found to present fewer advantages than others.

The *zenith telescope*, or equal altitude instrument, was first applied to the determination of latitude by Capt. A. Talcott, late an officer in the United States army; and it has been remodelled and specially adapted to the purpose in the Coast Survey, where it has become the favorite instrument on account of the great number and accuracy of the results that can be obtained in a given time, and the facility with which it is used. It is employed in determining latitudes by measuring, with an attached micrometer, the differences of zenith distance of stars culminating within a short time of each other, at nearly the same altitude, on opposite sides of the zenith. From the star catalogue of the British Association may be selected any desirable number of pairs of stars having the required relative positions. If, after bringing the micrometer wire of the telescope to a star at its meridian passage south of the zenith, and revolving the instrument about its vertical axis, the telescope remaining at a fixed inclination, we find that a star culminating north of the zenith passes precisely on the same wire, we infer that the zenith is exactly midway between the two stars, and consequently that its distance from the ce-

lestial equator, or the latitude of the place, is equal to the *mean* or half sum of the declinations of the two stars observed. If, furthermore, the second star does not pass precisely on the wire, but still in the field of the telescope, we are able to measure the difference of zenith distance by means of the micrometer, and correct by half that amount the half sum of the declinations. By means of a delicate level attached to the instrument we keep account of its deviation from the true vertical. The whole observation thus consists of pointing and reading the micrometer, and noting the level, in each position of the instrument.

This method possesses also the great advantage that the result is nearly free from the effects of refraction, which enters only so far as the difference of zenith distance is affected by it: the correction for refraction never exceeds a quarter of a second.

The observations with the zenith telescope being considerably more accurate than the positions of the stars in the British Association's catalogue, it has been preferred rather to increase the number of pairs employed, than to multiply observations on the same pairs. In practice, from twenty-five to forty pairs of stars are observed, at a station; with from four to six observations on each pair, on different nights. The probable error of a single observation with the zenith telescopes used in the Coast Survey is from three to five tenths of a second; while the probable error of the catalogue places considerably exceeds one second. The Greenwich observations, and other recent catalogues, frequently supply better places than the larger catalogue just referred to; and thus contribute to improve the result.

Prof. Bache has at various times communicated to the American Association notes on the use of the instrument in question, and at the Baltimore meeting read a paper showing the remarkable result that when at a given station the same stars are observed by the same observers with different zenith telescopes, and by different observers with the same instru-

ment, results almost identical were obtained; setting at rest the question of any residual personal or instrumental errors. It may safely be predicted that this simple and effective instrument is destined to supersede all others for the determination of latitudes in the field.

The *zenith sector* of the Coast Survey is constructed on the plan designed by Mr. Airy, the Astronomer Royal of England, and is similar to the one recently used in the British Ordnance Survey, but possesses some improvements in the details suggested by the use of the latter. It is an admirable instrument, perfect in design, and in point of accuracy compares well with a zenith telescope of equal power; but its ponderousness and the greater labor attendant on its use make it less eligible for observation in the field. Its capabilities, however, are much greater than those of the zenith telescope, inasmuch as it measures absolute zenith distances. It has been used with great success at six of the principal stations of the survey, and will be found described and figured in one of the forthcoming volumes of the Coast Survey Records and Results.

Irregularities of the Earth's Figure. — When the latitudes observed at different stations are referred to one another, by means of geodetic differences of latitude, there are discovered certain discrepancies, much larger than could result from the probable residual errors of observation — discrepancies, therefore, which can only be ascribed to local irregularities in the figure and density of the earth, and which have been called *station errors*. They are similar to those which are caused by the proximity of mountains; but they occur where the external configuration of the earth's crust would not indicate the probability of their existence. Prof. Bache recognized this source of error as early as the year 1844; and as a consequence, he determined to carry a continuous series of astronomical stations through the primary triangulation, in order to arrive as nearly as possible at the true figure of the portion of the earth's surface on which he was engaged.

The elements of the figure of the earth used in these comparisons are those obtained by Bessel in his last discussion of the results of ten measures of arcs. It is an interesting fact, which it is here in place to mention, that the observed difference of latitude of the extremities of the arc at present comprehended under the connected triangulation of the American Coast Survey, agrees very closely with the geodetic difference as computed from Bessel's elements. This arc extends from Maine to Maryland, over about five degrees of latitude and six and a half of longitude; in intermediate portions of its length, the deviation from the mean ellipsoid reaches three seconds of arc and over. The laws which govern these irregularities are still the subject of investigation. In the eastern portion of the survey, where the earth's crust has been more disturbed, the average amount of station error is between one and two seconds of arc; but it amounts to only about half a second in the sections south of the Delaware, where the diluvium rests apparently undisturbed.

Observations of Azimuth. — Observations of azimuth are made at all the latitude stations. The instruments used are the same as those employed in the measurement of the angles in the primary triangulation. The meridian is determined by observations made on the pole-star or other close circumpolar stars, principally when the object is near its greatest elongations. The motion of the star in azimuth at these points of its revolution being almost imperceptible, the result will be practically independent of the time. Moreover, if observations be taken within forty-five minutes on either side of the elongation, they can be reduced to that position by a most simple formula. The pole-star, however, even at its culminations, moves so slowly in azimuth, that the liability to error in pointing is found to be no greater at those periods than at the elongations; and Mr. Bache has finally pursued sometimes the plan of observing the star at equal intervals on both sides of the meridian before and after the culmination. When this method is employed, the reduction is simplified into a mere

taking of means: and it may here be noticed, that in these as in all analogous processes, it has been the aim of the superintendent so to arrange the methods of observing, as to diminish as far as practicable the labor and liability to error of the computations. The azimuths are liable to irregularities similar to those which have been described as affecting the latitudes: and a comparison of azimuths observed at different stations, extreme and intermediate, has manifested the existence of station errors among them, which can only, like the former, be referred to local variations of level.

OBSERVATIONS OF TIME AND LONGITUDE. — Observations of local time are made in connection with those of latitude and azimuth; the instruments employed being generally portable transit instruments of from twenty-six to forty-eight inches focal length, constructed by Simms of London and Wurdemann of Washington. For time-keepers, chronometers by the best makers are used, except for telegraphic determinations of longitude, when astronomical clocks are employed.

In the plan of reorganization of the survey, in 1843, it was "urged, and deemed essentially necessary, that the difference of longitude between some main points of the survey, and the meridian of any or all of the European observatories be ascertained immediately." In order to carry into effect this important injunction, Prof. Bache has availed himself of every means which science could afford. All the observations obtainable of eclipses, occultations and moon culminations made in this country previously to 1844, together with the simultaneous observations made abroad, so far as they could be found, have been collected and reduced; and similar observations have since been continued at Cambridge, Philadelphia, Washington, Charleston, and Cincinnati. All these stations being connected by the electric telegraph, their differences of longitude have been determined with great accuracy; and all the results are referred to Cambridge as a common station of reference.

The difference of longitude between Cambridge and Liver-

pool has also been determined by means of large numbers of chronometers carried repeatedly between the two stations on the Cunard steamships. These chronometric expeditions, in the words of Mr. W. C. Bond, director of the Harvard University Observatory, "for the magnitude and completeness of their equipments, have not been equalled by any of the similar undertakings of European governments. Even the 'Expedition chronometrique' of Struve was on a scale much less extensive." The voyages were continued through a number of successive years. The first great special expedition took place in 1849, and the most recent in 1855. In the latter the effect of temperature on the rate of the chronometers formed a subject of special investigation. For each instrument the effect of temperature on its rate was ascertained by experiment, and the average temperature during each trip was kept account of by means of a thermometric chronometer, constructed like the others, but with undivided balance, so that its daily rate was affected by six seconds for a change in temperature of 1° Fahr. Fifty-two chronometers were employed in this expedition, and were transported six times between Cambridge and Liverpool; giving nearly 300 individual longitude determinations, with a probable error of the mean result of two-tenths of a second of time. Yet this result differs by more than a second of time from that of the expeditions of preceding years, giving $4^h\ 44^m\ 31^s.8$ for the longitude between Greenwich and Cambridge, while the former expeditions had given $30^s.6$. The results by the astronomical methods also differ, eclipses and occultations giving $29^s.5$, and moon-culminations $28^s.5$. The results by the latter method are doubtless liable to large constant errors. Indeed, each method appears to be affected by sources of constant error which no accumulation of results will eliminate, and a greater approach to the truth must be sought by multiplying methods as far as practicable.

It is to be hoped that a submarine telegraph will ere long afford another, and perhaps very accurate, method of verifying

these results, and determining the longitude within the narrowest limits of error.

The differences of longitude between Cambridge and the principal stations of the survey in other sections are determined by the aid of the electric telegraph, wherever this has been established. In this method, which is by far the most accurate for determining differences of longitude, the Coast Survey has taken the lead, and has brought it to a great state of perfection, which subsequent operations of a similar nature executed in Europe have not yet reached. The idea of comparing the local time of different places by means of the electric telegraph is sufficiently obvious, and dates from the conception of the telegraph itself; but the refined methods by which the intervention of human senses and operations, and the consequent liabilities to error, are, in the greatest possible degree, avoided, and by which the time of transmission is measured and eliminated from the longitude, have been the result of careful study and long experience. The method of recording observations of time on a chronographic register, by means of a galvanic circuit, known in Europe as the *American method*, originated in the Coast Survey with the first attempts to determine longitude by means of the electro-magnetic telegraph. The *chronographic record* is made on a cylinder, revolving with nearly uniform velocity, covered with a sheet of paper, upon which a pen traces a line, interrupted or deflected for an instant, through the agency of an electro-magnet, every time the pendulum of the clock passes the vertical, and, in doing so, interrupts a galvanic circuit. Either cylinder or pen is at the same time slowly moving lengthwise; so that the line formed is a long spiral, which is thus graduated into spaces corresponding to seconds of time, and described with uniform velocity. When any instant of time is to be recorded, the observer strikes a finger key, which also breaks the galvanic circuit, and causes a similar mark to be made on the record; the position of which, in reference to the adjacent seconds marks, can be read off with great precision. In the

chronographs employed in the Coast Survey, a second is generally represented by from one half to three quarters of an inch, the cylinder being regulated so as to make one revolution in half a minute. By an ingenious arrangement in the clock, the break for every sixty seconds or minute is omitted, and every five minutes two breaks are omitted. By this means, a whole sheet may be read off without any other note than the time of beginning and ending.

The method of determining longitudes by means of the electric telegraph is substantially, and in brief, as follows: — A transit instrument, astronomical clock, and chronograph, is mounted at each station. After suitable observations for instrumental corrections at each station, which are recorded only at the place of observation, the clock at the eastern station is first put in connection with the circuit, so as to write on the chronographs at both stations. A number of stars, culminating near the zenith of the two stations, are selected by the observers. As they appear first upon the eastern meridian, their transit is recorded upon the chronographic registers at both stations by the observer striking the finger key. After an interval of time equivalent to the difference of longitude between the two places, which is measured by the clock, the same stars appear on the western meridian, and the observer at that station records this transit precisely as the other had done; and the difference of the two records of time is the measure of the difference of longitude.

It will be observed that these records have been obtained at both stations; and a little reflection will show that if there be any sensible interval of time consumed in the transmission of the signals, the difference of longitude obtained from the record at the eastern station will be too great by that interval, and that at the western station will be too small by the same amount. The mean result will give the longitude free from this error, and the difference will measure the time of transmission of the signals through the whole circuit.

Ten stars are generally exchanged with the eastern clock in

the circuit, and, after the first five, the transit instrument is reversed, so as to eliminate any residual error in the correction for collimation. The western clock is next put on, and the same operation repeated with ten other zenith stars. Not only is the result improved by the accumulation of individual results, but the advantage is gained, that the interval is measured by another clock, and the time of transmission eliminated in the inverse order of effects. The transits of the stars are generally recorded over fifteen wires. After the exchange of signals by the second clock is completed, local observations for instrumental corrections are again made, which conclude the night's work. These operations are repeated on at least three different nights; after which the observers and instruments exchange places, so as to eliminate the possible errors arising from causes connected with their individual peculiarities.

By the perfect and admirable method just sketched, we are able to measure arcs of longitude with the same degree of accuracy with which arcs of latitude have heretofore been ascertained; and a new element has thus been introduced into Geodesy. Since the general introduction of the electric telegraph and the development of the American method of longitudes, it has been applied to many of the older European geodetic surveys; and in general a very full acknowledgment has been made of their indebtedness to American science by the eminent geometers having charge of such works, with the single exception of M. Le Verrier, who, in his report to the Academy of Sciences, has made use of the earlier methods of the Coast Survey, since abandoned for others more perfect, without any mention of their origin. It was one of the earliest discoveries resulting from the telegraphic determinations of longitude, that the time of transmission of signals between stations several hundred miles apart is quite sensible, and that it appears in a great degree to depend on the distance. On the telegraph lines used in this country, where large iron wire is employed, and the circuit is completed through the earth,

the rate of transmission has been found to be from 11,000 to 20,000 miles per second. Whether this actually was the velocity of the galvanic current, or whether it rather measures the time during which the current must be established in order to produce the effects that we observe, is, for the present, an unsolved question.

The *telegraphic* determinations of longitude have been extended from Washington northward to Philadelphia, New York, Cambridge, Bangor, and Halifax, and southward to Petersburg, Wilmington, Charleston, Savannah, Mobile, and New Orleans.

Great credit is due to the public spirit of the telegraph companies, who have extended every facility to the operations of the Coast Survey, and have given the use of the line, after the working hours, free of charge.

In sections of the coast to which the telegraph has not yet penetrated, such as Florida, Texas, and the Pacific Coast, the longitudes of cardinal points are determined by observations of moon culminations, and by chronometers. Corresponding observations of moon culminations are made at several American observatories, at the expense of the Coast Survey; and most valuable aid is derived from the series of meridian observations of the moon made at Greenwich, which are always most promptly obtained through the kindness of the Astronomer Royal. The manner in which the reductions are made to keep pace with the observations at the latter observatory is admirable, and worthy of general imitation.

The chronometric determination of longitude between Savannah, and Fernandina, in Florida, the details of which were communicated to the American Association at the Montreal meeting, may serve in plan of execution and mode of discussion, as a model for operations of a similar character.

Observations on the Magnetic Elements. — Observations of magnetic declination, dip, and intensity are made at all points where it is desirable for purposes of navigation that the declination should be known; and also at many stations

of the primary triangulation, generally those where astronomical observations are likewise made.

The instruments employed are similar to those used in the British magnetic surveys; being declinometers and horizontal force magnetometers on the plans of Gauss and Webber, Lloyd and Lamont, by Jones, of London; and dip circles by Barrow and Gambey, provided with Lloyd needles for total intensity. In this manner are obtained not only the variations of the compass required for the charts, but important contributions also to our knowledge of the earth's magnetism, which have already borne fruit in the construction of a magnetic chart for the United States. This subject, one of great scientific importance, will be found referred to more at length in the second part of this report.

DETERMINATIONS OF LEVEL. — The heights of the trigonometrical stations above the level of the sea are determined, generally, by observations of reciprocal zenith distances, which observations are frequently checked by direct levellings from the ocean. Interesting comparisons are in progress between the results obtained, and those which are furnished by barometric and thermometric measurements.

PLANE-TABLE TOPOGRAPHY. — The topographical survey is executed with the plane-table. The outlines of the shore, the irregularities of the surface, the forms and dimensions of hills, forests, streams, rocks, meadows, towns, and villages, are all represented by certain conventional modes of drawing. The topographical maps are generally surveyed on a scale of $\frac{1}{10000}$ of the natural dimensions. In localities where a great amount of detail is to be represented, such as large cities and their vicinity, a scale of $\frac{1}{5000}$ is employed, while on flat and thinly settled ranges of the coast a scale of $\frac{1}{20000}$ is used. The extent of ground represented upon a single topographical sheet depends upon the scale; on a scale of $\frac{1}{10000}$, or about 6 inches to the mile, a square foot of the drawing represents about 4 square miles of the surface of the earth. The size of the sheets is generally 30 inches by 48.

On the sheets drawn in the field, the irregularities of the surface, — the hills and depressions, — are indicated by curves, or *contour lines*, representing the intersections, with the natural surface, of a system of horizontal planes, one above another, at equal distances apart, measured vertically. In the reduced drawings for the engraved charts, the spaces between these horizontal lines are filled up with *hachures*, the darkness of the shade being made proportional to the steepness of the slope, according to a system somewhat modified from that of Lehmann. Scales of shades have been printed, showing the distance between the hachures and the strength of stroke adapted to different scales of the maps. The absence, in general, of very abrupt slopes, and the frequency of gentle ones, rendered necessary the modification of the Lehmann system. The topographical survey is carried as far inland as is required for purposes of navigation, and for the defence of the coast.

The Hydrographic Survey. — Next in order, and based upon the positions furnished by the triangulation and topographical survey, comes the hydrography. This department of the survey supplies the results which are of most universal interest to the public, and of which the practical utility is most easy to be understood. By a thorough system of hydrographic survey, the configuration of the bottom of the ocean is mapped out, new channels are discovered, hidden dangers are detected, the situations of rocks and shoals which were but imperfectly known are determined with accuracy, the peculiarities of the tides and of the direction and velocity of tidal currents are made apparent, and the most suitable positions are determined for the planting of buoys and the erection of beacons and light-houses.

*Soundings* are taken with such frequency as to exhibit accurately the variations of depth of water; the number of casts of the lead depending on the degree of irregularity of the bottom, or its greater or less declivity. The casts are determined in position relatively to the trigonometrical stations, generally by means of angles measured with the sextant at the place of

sounding, and sometimes by the use of two theodolites on shore. In soundings off the coast, and in cases where extreme accuracy of position is necessary, as in the determination of sunken rocks, the latter is the method preferred. The position of the sounding vessel is not, in practice, determined at every sounding, but at every sixth or tenth, according to frequency; and the intermediate soundings, being taken at equal intervals of time, are laid down at equal intervals between the positions determined by angles.

The soundings extend from the shore, and from the head of tide-water generally, as far seaward as the exigencies of navigation require. One of their principal uses being to assist the mariner in determining his position, it is desirable that the record should exhibit not merely the depth, but the character also of the bottom. The sounding leads are, accordingly, provided with an apparatus designed to bring up along with it a portion of the sedimentary material which it encounters; and specimens of the varieties of this material from different localities are preserved among the collections of the Coast Survey, and the respective characteristics of the bottom are indicated on the charts.

Microscopic examinations of many of these specimens have shown them to be crowded with organisms, of which the number and the *genera* vary with the depth; and hence it is not impossible that science may discover, in the laws governing the distribution of microscopic forms in the bottom of the ocean, an additional means of assisting the mariner to determine his position at sea.

Various improvements in the apparatus for bringing up these specimens of the bottom have been introduced in the course of the survey, invented by naval officers while prosecuting these researches: among them may be mentioned Stellwagen's attachment to the lead, — a metallic cup, with a circular leather valve, closed by the pressure of the water in being drawn up, — a contrivance in common use, and answering very well the purpose intended, in moderate depths. For

greater depths, Craven's specimen-box, with a metallic hinged valve or lid, and Sand's specimen box, with a sliding valve closed by the action of a spiral spring, are used with success. For soundings exceeding 100 fathoms in depth, Massey's self-registering sounding machine is used in connection with the lead and line, with several improvements suggested by its use.

The hydrographic sheets are drawn on scales varying from $\frac{1}{5000}$ to $\frac{1}{40000}$, the more usual scales being those of $\frac{1}{10000}$ and $\frac{1}{20000}$.

Observations on the Tides. — In order to reduce all the soundings to the depth at mean low-water, the level of which is assumed for the plane of reference, observations of the tides of sufficiently long duration for this purpose are made by the hydrographic parties as part of the programme of their work. These are in connection with the system of more long-continued observations made at selected points of the coast, where tidal registers have been kept up with great attention and perseverance, for the purpose of ascertaining, upon the basis of extensive observation, the complicated laws governing the tides in the different seas which wash our shores. The observations have been made half-hourly, at a number of stations, for various periods of from one to three years. At intermediate stations, half-hourly observations during two lunations are found to be sufficient to determine the constants.

A self-registering tide-gauge is much used, by which a continuous curve representing the successive changes in the height of water is traced on paper moved by clock-work, by a pencil actuated by the rising and falling of a float in a vertical box, to which the tide has free access. The superintendent engages personally in the discussion of this most interesting subject; and he has made several communications to the Association to whom this committee owe their appointment, on the nature of these researches, and the results attained, which are to be found among the published transactions.

This subject is the more important from the fact that the tides of the Atlantic, the Pacific, and the Mexican Gulf coasts

of the United States present three distinct types. The diurnal tide, which is moderately large on the Atlantic coast, and produces a diurnal inequality in height and time which is perceptible without being excessive, is very great on the Pacific; and in the Gulf of Mexico its predominance is so decided that the semi-diurnal tide is almost obliterated.

In addition to these investigations, which relate to the general phenomena of the tides, careful observations are made upon the direction and velocity of the *currents* which the tides produce. The information thus obtained is of no less importance to the mariner, than a knowledge of the amount of rise and fall of the water. It is accordingly embodied in the charts, along with those other matters of varied information which have been enumerated as essential to the safety of navigation; and thus constitutes the final contribution to that fund of positive knowledge of the sea and its bed, of its changes and the laws that govern them, on which the commerce of the country so largely depends for its safety.

EXPLORATION OF THE GULF STREAM. — A hydrographic survey of our coast would be incomplete, if it did not embrace the investigation of the remarkable ocean current which divides the waters adjacent to our Atlantic coast from the wide ocean beyond. The importance of the exploration of this stream is recognized in the original law of 1807, which extended the scope of the survey so as to embrace it. On assuming charge of the work, Prof. Bache at once organized a system of observations for the purpose of determining its limits and principal characteristics.

Previously to that time, casual observations of the stream had been made by private and public vessels of England and America, the results of which were collected by Major James Rennell, F. R. S., and published in his work on the currents of the Atlantic ocean. These observations were confined to the surface, and gave only vague results.

The method of exploration adopted by Professor Bache was, to determine the limits of the stream by the temperature of

its waters at all depths, by means of deep sea thermometers, along lines perpendicular to its several directions. This work was commenced in 1845, and has been continued up to the present time. Sixteen sections have been run across the stream at different points of the coast, along which the temperature of the water at various depths has been determined, and also sections of the bottom, wherever it has been found possible to obtain soundings.

For these explorations thermometers are required specially adapted to the purpose. Ordinary thermometers would indeed suffice for observing the temperature of the surface, but in order to obtain that which prevails at some depth, it is necessary that they should be self-registering, and able to withstand the crushing pressure of the water. They must be of simple construction, not liable to derangement by ordinary shocks, nor affected in their indications by the enormous pressure to which they are exposed; they must readily acquire the temperature of the water, which should have free access to all parts, and they must therefore be made of materials not liable to corrosion by salt water. After many experiments on a variety of instruments, as Rutherford's, Six's, Breguet's, and Jurgensen's, the metallic self-registering thermometers invented by Saxton, the assistant of Prof. Bache in the office of weights and measures, was found best to fulfil the above conditions, and it has superseded all others in use. The statistical table shows that over 3,400 casts have been made and registered for depth and temperature of the Gulf stream. The results arrived at from the discussion of these observations will find mention in the second part of this report.

The work is still in active progress; and it is much to be desired that the exploration eastward from the American side of the northern part of the stream, should be met by a corresponding exploration from the shores of Great Britain westward, which would complete our knowledge of the Gulf stream, throughout its whole extent from the shores of Florida to those of Ireland.

OPERATIONS ON THE PACIFIC. — Within the past few years the Pacific coast of the United States has suddenly attained an extraordinary commercial importance. This coast had previously been but little visited, its population was scanty, and its geography was almost wholly unexplored. In the absence of any charts worthy of confidence, the navigators of the bordering ocean were subject to the greatest inconvenience and exposed to the greatest dangers. In this state of things, the sudden starting into existence in that distant portion of our territory, of a great and wealthy state, almost wholly dependent upon the sea for its communications with the rest of the civilized world, created a pressing demand for the prompt extension into that remote region, of a work so indispensable to the prosperity of a community thus completely isolated, as the survey of the coast. This demand was met by the superintendent, with an energy not the less honorable that it is eminently characteristic. Perceiving at a glance all the urgency of the case, he set on foot a rapid examination of the coast in its entire extent; aiming to secure at once the results most practically important, by determining the geographical positions of the principal headlands, executing the most important lines of sounding, and exploring the entrances to harbors of commerce and of refuge. It was a concession to necessity, to substitute methods approximately correct, for those refined and severely accurate processes of which the details have been given above; and to allow *reconnaissance* for a time to take the place of exact survey. By proceeding in accordance with these principles, it was found practicable to furnish to the world the information of most indispensable importance in relation to the Pacific coast, in a surprisingly short space of time. This field has proved of all the most prolific of discovery. Every step has brought to light some important geographical fact unknown before; but to attempt a recapitulation of examples, would be almost to rehearse the whole progress of the work. The table of published charts in the second part of this report bears witness to the success which has attended these efforts,

notwithstanding the great difficulties encountered from want of facilities and exorbitant prices.

OFFICE-WORK AND PUBLICATION OF CHARTS. — The results of the field-work are prepared for publication at the office of the Coast Survey, in Washington City. The reductions of geodetic and astronomical observations are, in the first instance, made by the observers themselves; but a second computation is independently made in the *computing division* of the office by persons having no connection with the field-work. Strict scrutiny of the observations, and perfect security against errors of computation, are thus insured; the two computations being examined and compared by an experienced person, who refers whatever discrepancies may appear to exist to the superintendent.

Of the formulæ and methods employed in the computation, not even a sketch can be given in a general review like the present. It will be sufficient to state that in this, the purely mathematical part of the operations, as marked improvements have been made as in any other branch of the work. The processes are highly systematized, and partake of the highest refinements of modern science, as is evidenced by the systematic application of the method of least squares in the discussion and adjustment of results.

The construction of the maps and charts for publication commences in the *drawing division* of the Coast Survey office.

The projections of the meridians and parallels are made according to the system of *polyconic development* of the earth's surface, of which special accounts, with accompanying tables, may be found in the C. S. reports for 1853 and '56. On these projections, the positions of the triangulation points are laid down according to their latitudes and longitudes, obtained by the geodetic operations. On the sheets covered by the topographical and hydrographic surveys, the triangulation points are likewise laid down, with reference to similar projection lines, and the reductions to the scales of publication are thus

made with extreme accuracy by indefinitely projecting the controlling points of the survey, and drawing in the details by means of small squares, or by the pantograph. The *camera lucida* has sometimes been employed, but is found too trying to the eyes of draughtsmen. It may be mentioned here that experiments are in progress having in view the application of photography to the reduction of the maps, and that there is good reason to hope for successful results.

The maps and charts emanating from the Coast Survey are divided into two general classes — viz.: *preliminary* or provisional charts and sketches, issued with as much expedition, after the individual surveys, as is consistent with accuracy of general portraiture, and designed to supply the more immediate and pressing demands of commerce; and the *finished charts* proper, which must embody all the information furnished by the survey, even of the minutest detail, and which ought at the same time to be executed in the style of the highest existing artistic excellence.

The charts are various in character, according to the immediate objects which they are designed to subserve. Thus, one description of chart has for its use to convey to the navigator a just notion of an extended coast line, and of the relative positions of objects most conspicuous at a distance, so as to enable him to identify his position as he approaches from the open sea. Another gives him the minuter information which he needs when the purposes of his voyage bring him for any reason closer in-shore. They are, therefore, classified under the three heads of *general coast charts*, *coast charts*, and *charts of harbors, anchorages, &c.*

The *general coast* (or off-shore) *charts*, on the scale of $\frac{1}{400000}$, represent the shore-line of the coast and its general topographical features, so as to be readily recognized by the navigator approaching it. Omitting minute details, they show at a glance the contour of the shore, the location of the islands, shoals, and rocks which the navigator may encounter in approaching the main land, the soundings off-shore, selected in-

shore soundings, and finally the principal natural and artificial landmarks.

The *coast* (or in-shore) *charts*, on the scale of $\frac{1}{80000}$, and the *harbor charts*, on various scales, from $\frac{1}{5000}$ to $\frac{1}{80000}$, present with minute accuracy every natural feature or other object above or beneath the water which can be introduced without producing confusion or indistinctness. In these charts, the hydrography is so elaborately displayed as to indicate, by the shades of depth, the fathom lines and the soundings, the safe route for a vessel of any given draught, and, as a necessary consequence, to exhibit, at sight, the principal channels by which every important place may be approached. The lines of range of natural landmarks, or of lights and beacons, the appearances of different light-houses and the characters of their lights, with all other indications by which channels are signalized to the mariner, are also everywhere given in the fullest manner. They further contain sailing directions designed to furnish information in entering harbors, graphic views of important landmarks, and sketches of the position and characteristics of light-houses, beacons, and buoys — such as may serve to assure the navigator of his true position, whenever any of these objects are in sight. The topography of the shore and of the land for some distance from the shore is furthermore minutely given, for various reasons, some of which have been hinted at, and of which others will be alluded to hereafter.

Of the *general coast charts* ($\frac{1}{400000}$), it is estimated that the Atlantic and Gulf coasts will be covered by sixteen sheets.

The scheme of *coast charts* ($\frac{1}{80000}$), planned for these portions of the coast, embraces one hundred and thirteen sheets.

The Pacific coast is as yet too little known in its minute features to afford a basis for a definite estimate.

Besides these *finished charts*, which, from their nature, require a lengthened period for the collection of the data from which they are compiled, and are slow of execution, — their elaborate details requiring the highest skill in drawing and engraving, — there is issued a series of *preliminary charts* and

*hydrographic sketches.* These, embodying information which it is desirable to make accessible at the earliest available opportunity, are prepared and published as soon as possible after the completion of the work in the field. For the principal charts of this latter class, the scale of $\frac{1}{200000}$ has been adopted; they are less elaborately finished than those before described, and are designed to meet the wants of the public, until the more perfect maps can be made ready for issue. They also enable the office department of the survey to keep up with the operations in the field and afloat: each season's work as it is turned into the office is drawn and engraved on the plate, and, when required, a new plate is made by the electrotype process, large enough to retain the old and receive the new work. Of the *preliminary sea-coast charts*, it is estimated that the Atlantic and Gulf coasts will require thirty-three sheets.

When the charts are drawn and verified, they pass into the *engraving division* of the office, where they are executed on copper by a corps of engravers, among whom there are some artists of distinguished merit. Here, as in every other department of the work, the division of labor is productive of economy in time, combined with superior excellence of results. The engraving of outlines, of hill-topography, of the dotted shading of the sea-bottom, of the figures denoting the soundings, and, finally, of the lettering, are so many different branches of the art, executed by different persons, who have, by talent or practice, acquired superior skill. When the plates are completed, copies are taken of them by the *electrotype* process, which are used for printing the charts, while the original plates are preserved among the archives.

The electrotyping establishment of the Coast Survey is very extensive, and is one of the most successful in the world. It was in practical operation several years before the process came into general use, and considerable advances were made in the art at this establishment. The method of *iodizing* the matrix, which effectually prevents adhesion without interfering

in the least degree with the perfect sharpness of the cast, was there discovered, and, by an admirable system of experiments, the best size and arrangement of batteries, the proper temperature of the solutions, and all other conditions necessary to produce the best, most uniform, and cheapest results, were developed. The practical use of the process is not confined to the production of copies for printing. The division of labor in engraving the charts is often very much facilitated, and great despatch in their production is attained, by dividing, and executing them in two, or even four, separate pieces, which are afterwards joined on a common electrotype plate.

The charts are finally printed at the Coast Survey office, and distributed to sale agents in the principal seaports. The prices are fixed at very low rates, varying from fifteen to seventy-five cents per sheet, so as to place a complete set of sheets within the reach of every navigator. Copies are also freely distributed to seminaries of learning, public libraries, and other institutions of a similar character.

PUBLICATION OF OBSERVATIONS AND METHODS. — The importance of an early publication of the observations made in the progress of the Coast Survey, and of the methods practised in the execution and computation of the work, has been repeatedly urged upon Congress by the superintendent; and means were appropriated towards that object in 1854–55, and in the two subsequent years. It has been the reproach of works of this kind, that the results only are furnished, while the data from which they are derived are not presented for scrutiny at all, or if at all, only so late that for practical use they have lost much of their value. The publication of the archives, while it secures the work against loss by accident, and submits it to the criticism of the world, has the further very great advantage, that it insures full information from, and the full responsibility of, those by whom the work is executed.

Considerable progress has been made in this undertaking, as appears from the account given in the annual report of the superintendent for 1856 and 1857; where also the plan of

publication is developed, of which a brief sketch must suffice here. The order of arrangement for the geodetic work is at first necessarily chronological, the volumes for each year comprising the records and results of all the various classes of work; while if prepared only after the completion of the survey, the volumes would most naturally arrange themselves according to the different operations. This necessity being kept in view, the plan has been carefully laid out so that the matter contained in the annual volumes may ultimately be divided, and rearranged so as to form volumes relating to separate subjects. The *record* of the observations is given in full, and being unchangeable, is kept separate from their reduction, of which the principal steps are given, and which may be liable to change from accumulation of data or from improvement of methods. As far as practicable, the record and the reduction of the observations are arranged on opposite pages. The volumes thus in course of preparation embrace, beside other subjects, the operations of Geodesy, Chronometric Longitudes, Telegraphic Longitudes, Explorations of the Gulf Stream, and Sailing Directions. It will be remembered, that, in addition to the great variety of interesting communications on subjects connected with the Coast Survey, made to the Association at the Montreal meeting, by Prof. Bache, it was the exhibition to the members of a volume of proof-sheets of the geodetic volume, which led to the appointment of the present committee. This committee are therefore compelled to express the deep regret which they have felt, on learning that the publication of the work has been unavoidably suspended for want of the necessary appropriation to carry it forward; and their earnest hope that, when the present temporary embarrassment of the public treasury is relieved, the means will be supplied for its speedy consummation. The publication of the observations and methods will be hailed with great satisfaction by men of science, both at home and abroad; while the volumes relating to the harbors and coast will have nearly the same interest for commercial men and navigators.

ORGANIZATION AND ECONOMICAL MANAGEMENT OF THE COAST SURVEY.— The whole work is under the administrative direction of the Treasury Department of the United States. The superintendent arranges the plan of conducting the work, and issues instructions for its execution to his assistants; he directs and is responsible for the accuracy of all the scientific parts of the work, and controls the expenditures, making an administrative examination of the accounts, which is a great source of economy. Besides performing these duties of superintendence, he engages personally in observations in the field, and in those able discussions of the results of which the pages of our scientific journals contain ample evidence.

The corps of assistants is composed of three classes,—civilians, officers of the army, and officers of the navy. The civilians, many of whom have been trained in the survey, entering as cadets, or, as they are designated, *aids*, and rising by merit to higher grades, form a nucleus of a more permanent kind. They preserve uniformity in the methods of the survey, and can be called upon freely for duty of every kind. The officers of the army and navy are detailed by the heads of their respective departments, on application of the superintendent; they are liable to be changed frequently, as the exigencies of the military or naval service require. Receiving no extra emolument for their services on the Coast Survey, their positions are only valuable to them under peculiar circumstances, which change frequently, and there is a limit, therefore, to the calls upon them for service. Without the nucleus of civilians the whole work might be disorganized by calls for the services of the army and navy officers in their immediate professional line. On the breaking out of the war with Mexico, all the officers of the line of the army, and part of those of the staff, serving on the Coast Survey, were detached from it. The harmonious coöperation of these three classes of persons, which has been maintained during the past fourteen years, is highly advantageous to the country, and could not, it is obvious, be secured except under the direction of a distinguished civilian;

since questions of rank and authority between officers of the different branches of the service would inevitably interfere disastrously to the best interests of the work, if its direction were confided to either the War or Navy department; and if the operations on land and at sea were carried on under distinct organizations, there would result a want of coöperation as to time, methods and objects, by which the hydrographic survey would fail in a great measure to derive from the land-work those elements of accuracy which are essential to its perfection.

The progress of the Coast Survey, from year to year, is communicated to Congress in the annual report of the superintendent. Besides an account of the extension of the work in the different sections, accompanied by sketch-maps illustrating the same, these reports contain, as an appendix, the preliminary maps, charts and sketches produced during the year, and valuable contributions to knowledge in the form of scientific discussions of various subjects connected with the survey, such as tides and terrestrial magnetism, and of new methods of observation or computation developed by the persons engaged in the work. The high character for interest and usefulness which these reports sustain is too well known and universally acknowledged, to require commendation. With wise liberality Congress has printed large editions of them for general diffusion, and the distribution list shows that they are in great demand not only on the seaboard, but also in the interior States, evincing the gratifying fact that large numbers of citizens other than those immediately engaged in commerce and navigation feel an appreciative interest in a work designed for the benefit of the whole nation.

It may not be inappropriate in the general sketch which we are taking of the actual operations of the Coast Survey, to observe that the *economy* which has marked the conduct of this national work has been no less remarkable than the comprehensiveness of its scope, the rapidity of its execution, the scientific exactness of its processes, or the substantial value of

its results. It may very safely be said that no other government has hitherto succeeded in securing in this branch of its service, results so large and so valuable, in so little time, and at so little expense. This fact is no doubt in a great measure due to the wisdom with which the work has been laid out, to the administrative ability which has secured activity and efficiency in all its departments, to the division of labor, and to the personal ability or skill, as well of the subordinate members of the corps, as of the chief. But it is also, and in no less measure, due to the system of disbursement and financial control, and to the method of keeping the accounts, introduced by the present superintendent, which are judiciously devised so as to protect, in the most effectual manner, the public interests.

A careful study of the question of relative progress and expenditure, and a comparison of the cost of our national survey with that of the surveys of other nations, for which the data are given in a special report prepared by the superintendent in 1857, in compliance with a call from the Treasury Department, have led the committee to the conclusion expressed at the close of this report in regard to the economy of the present system. The details are of a character which would extend this report to an undue length, but the committee may express their desire that the document referred to be made accessible to the public through its publication by government.

# PART II.

## RESULTS.

### THEIR BEARINGS UPON THE NATIONAL WELFARE, AND UPON THE ADVANCEMENT OF SCIENCE.

In passing to consider the results actually wrought out in the progress of the survey of our coast, and the various important bearings of these, both useful and scientific, the Committee are aware that it is difficult to avoid the appearance of occasional repetition: since any sketch, however cursory, of the history of the work, or any description, however imperfect, of the processes employed in its execution, must, from the nature of the case, be in itself, to some extent, an anticipation of results; though these be mentioned only by way of allusion, or introduced purely for the sake of explanation, or of illustration. In so far, nevertheless, as may be practicable, care will be taken not to retread ground already beaten.

The valuable results which the Coast Survey has been, and still continues to be, the means of securing, are distinguishable into two classes, according as we restrict our view to the objects for the sake of which the Survey was expressly instituted, or extend it to those more varied interests which are incidentally, but no less positively, promoted by its operations.

For the sake of convenience, we will refer to these two classes of results under the heads, *direct* and *collateral*.

### DIRECT RESULTS. — BENEFITS TO NAVIGATION AND COMMERCE.

The direct results are the numerous and invaluable aids and benefits which the survey affords to navigation and commerce. These were the ends most immediately and distinctly contem-

plated in the origination of the work, and it is accordingly in the importance which attaches to them, that the main argument in favor of its prosecution to completion must always be found.

We may indeed dwell with just and honorable pride upon the services rendered to science, in its various branches, through the instrumentality of a work which can only be prosecuted by the aid of recondite scientific principles, applied in practice by accomplished and zealous scientific men. As physicists, the members of this Association may even, perhaps, regard with higher satisfaction the contributions furnished to our knowledge of nature, the improvements originated for us in methods of observation, and the ingenious mechanical contrivances invented for instrumental accuracy, which we owe to the men engaged in this great public work, and, through them, to the work itself, than our tastes may incline us to feel in view of the more material and immediately tangible results which flow from it; but we must not forget that the majority of mankind look upon all undertakings which involve expenditure to the public from a utilitarian point of view; and while, therefore, we may justly challenge the admiration of all men, whether they be professedly scientific or not, for results which we ourselves so greatly prize, yet we must not expect that all men will so far enter into our enthusiasm as to undertake such works for the sake of such results only. In the present case, fortunately, the directly useful results accomplished are so palpably great, that it will be sufficient to name them, in order to satisfy the most exacting economist in regard to the wisdom of the outlay by which they have been secured.

We therefore present now a summary view of the progress already made towards the completion of the work, followed by a condensed table, compiled from data obtained from the Coast Survey office, showing, in two parallel columns the work executed previous to 1844, under the superintendence of Mr. Hassler, and that from 1844 to 1857, inclusive, under the superintendence of Professor Bache.

PROGRESS, OR PROPORTION OF THE WHOLE COAST SURVEYED. — The survey has been extended into every seaboard State and territory of the United States, on the Atlantic, Gulf of Mexico and Pacific coasts, and is now in progress, either in field or office-work, in all of them.

For the purpose of estimating its progress towards completion, we may consider either the extent of general coast-line (omitting minute indentations), or the length of the shore, or water-line, including that of bays, sounds, islands and rivers, to be covered by the triangulation.

The general coast-line of the Atlantic (including Delaware and Chesapeake, as well as all open bays), is estimated, from recent measurements, made on the progress sketches, in the office of the survey, at 3,036 miles; that of the Gulf of Mexico, at 2,162 miles; and that of the Pacific (including the Straits of Fuca), at 1,866 miles; making, in all, 7,064 miles.

Of the foregoing, there have been already covered by triangulation, on the Atlantic coast, 2,372 miles; on the Gulf coast, 780 miles; and on the Pacific coast, 325 miles: making, in all, as appears in the following table of statistics, 3,477 miles.

A measurement of the shore-line (including bays, sounds, islands and rivers), covered by the triangulation, made in a similar manner, gives for that of the Atlantic coast, 14,723 miles; of the Gulf of Mexico, 10,406 miles; and of the Pacific coast, 4,252 miles: in all, 29,381 miles. Of this shore-line, the triangulation has already extended over, on the Atlantic coast, 10,787 miles; on the Gulf coast, 4,075 miles; and on the Pacific coast, 1,288 miles: in all, as appears in the table of statistics, 16,150 miles.

Each of these two modes of estimating the work already executed, and that remaining to be done, affords us this conclusion: that the Atlantic sections are nearly three-fourths done, and the Gulf sections one-third done.

Our knowledge of the minuter features of the Pacific coast being as yet in a much less advanced state than that of the

other two portions of the coast, renders a precise estimate for it comparatively less practicable; as far, however, as we can venture to determine in this matter, the Pacific section is not quite one fourth executed.

With the limitation just specified, taking the whole coast of the United States into consideration, the survey is carried forward about one half to completion. This estimate is based on the triangulation executed; and a similar consideration of the topography will lead us very nearly to the same conclusion.

The length of the general coast being taken, as above stated, it appears that the topography is executed over 1,889 miles in the Atlantic section, 764 miles in the Gulf section, and 259 miles in the Pacific section: in all, 2,912 miles, out of 7064 miles, showing two fifths of the whole. It is to be borne in mind, in making this estimate, that the topography is, in general, a whole season behind the triangulation, and that the hydrography thereafter follows in its turn.

Looking at these data of the field-work, and adding to our view the office-work consequent thereon, we are justified in considering that, at the rate of appropriation for the three years from 1855 to '58, and with the like efficient management, the survey of the Atlantic and Gulf coasts may certainly be essentially completed in from twelve to fifteen years. This agrees substantially with the latest estimates of the superintendent of the work.

Estimates of time for completion were, in the early stages of the survey, necessarily but conjectural, on account of the inaccuracy and looseness of the geographical maps of the coast then extant, the length of shore-line shown on them being greatly in defect; but, as data for a more and more close approximation to the true amount have become available by the operations of the survey itself, our information on this subject is now so much more detailed and reliable, that we may look with a tolerable degree of confidence on the conclusion at which we have arrived.

STATISTICS OF FIELD AND OFFICE-WORK OF THE UNITED STATES COAST SURVEY.

| | Previous to 1844. | From 1844 to 1857, inclusive. | Total from commencement to 1857, inclusive. |
|---|---|---|---|
| *Reconnaissance.* | | | |
| Area, in square miles, | 9,642 | 43,171 | 52,813 |
| *Base Lines measured.* | | | |
| Primary, number of, | 1 | 9 | 10 |
| Secondary, number of, | 2 | 41 | 43 |
| Length of, in miles, | 19½ | 133½ | 153¼ |
| *Triangulation.* | | | |
| Area, in square miles, | 9,076 | 31,949 | 41,025 |
| Extent of general coast in miles, | 570 | 2,907 | 3,477 |
| Extent of shore-line, in miles, including bays, sounds, islands, and rivers, | 1,588 | 14,562 | 16,150 |
| Geographical positions determined, | 1,183 | 5,789 | 6,972 |
| *Astronomical operations.* | | | |
| Stations occupied for azimuth, | 9 | 59 | 68 |
| do. do. latitude, | 9 | 100 | 109 |
| do. do. longitude, | 1 | 73 | 74 |
| *Magnetic stations.* | | | |
| Magnetic stations occupied, | 0 | 183 | 183 |
| *Topography.* | | | |
| Area surveyed, in square miles, | 6,039 | 8,507 | 14,546 |
| Length of general coast, | 414 | 2,498 | 2,912 |
| Length of shore-line, in miles, including rivers, creeks, and ponds, | 7,667 | 25,203 | 32,870 |
| Length of roads, in miles, | 12,239 | 11,394 | 23,633 |
| *Hydrography.* | | | |
| Soundings, number of, | 808,147 | 4,080,738 | 4,888.885 |
| Specimens of bottom preserved, | 1,029 | 5,972 | 7,001 |
| Soundings in Gulf-stream for temperature, | 0 | 3,429 | 3,429 |
| Tidal stations, permanent, | 20 | 275 | 295 |
| do. temporary, | 107 | 492 | 599 |
| Current stations occupied, | 0 | 783 | 783 |
| *Records.* | | | |
| Total number of volumes of observations and computations, | 566 | 8,485 | 9,051 |
| *Maps and Charts.* | | | |
| Topographical maps, originals, | 168 | 507 | 675 |
| Hydrographic charts, originals, | 142 | 457 | 599 |
| Total number of manuscript maps and charts, | 325 | 1,325 | 1,650 |
| Number of sketches made in field and office, | 311 | 1,189 | 1,500 |
| *Engraving and Printing.* | | | |
| Engraved plates of finished charts, number of, | 5 | 61 | 66 |
| Preliminary charts, sketches and diagrams, for Coast Survey annual reports, | 0 | 319 | 319 |
| Electrotype plates made, number of, | 0 | 400 | 400 |
| Finished charts published, number of, | 0 | 64 | 64 |
| Preliminary charts and hydrographic sketches published, | 0 | 258 | 258 |
| Printed sheets of maps and charts distributed from office, | 0 | 66,576 | 66,576 |
| Printed sheets placed in hands of sale agents, | 0 | 42,948 | 42,948 |

CHARTS PUBLISHED.— The charts which have been described, in their general character, in the first part of this report, exhibit such an exact and clearly intelligible delineation of the coast-line in its contour and topography, and of the adjacent waters off-shore, and in bays and inlets, and, to a certain extent, even in rivers, as to enable the mariner to choose his course with confidence, avoiding hidden dangers, and following the secure channels as easily as if the sea-bottom itself were exposed to his view. Or, rather, they are a safer guide than the sight of the bottom could be; since they enable him at a glance to take in a wide area on the chart, while a direct inspection could extend, even under the most favorable circumstances, only to a limited space in the immediate vicinity of his vessel. The number of such delineations, or charts, which the American Coast Survey has produced, is already very great: various in character, according to the immediate objects which they are designed to subserve. Of the finished charts, there have been published forty-seven sheets of charts of harbors, anchorages, and the like, to various scales, from $\frac{1}{5000}$ to $\frac{1}{80000}$; eight sheets of "in-shore" or coast-charts, to scale of $\frac{1}{80000}$; and one sheet of "off-shore" or general coast-chart, to scale of $\frac{1}{400000}$. Of the preliminary sea-coast and harbor charts, there have been published upwards of eighty sheets, to various scales from $\frac{1}{10000}$ to $\frac{1}{400000}$: and of hydrographic sketches, upwards of one hundred and twenty.

As regards the finished maps, it may be said that, in point of execution, they do honor to the government under whose authority they have been put forth. That they are characterized by the greatest precision, may of course be said, since this is the first and essential condition which gives to any map its value: but considered, also, as specimens of art, they are unsurpassed in any particular, whether in the drawing, engraving, or printing, by any works of their class ever produced by public or private enterprise, at home or abroad. There results from this the double advantage, that while their elaborate exe-

cution carries with it a certificate of their refinement and accuracy, and gives to the navigator who consults them assurance of their truth, it at the same time reflects credit upon the country, and presents a model for imitation which cannot fail to improve the standard of artistic execution in every work, whether of a scientific or of a purely æsthetic character, produced among our people.

Below is given a list of the finished charts already published, arranged in geographical order, from Maine to Washington Territory; also, a list, arranged in like manner, of preliminary charts and hydrographic sketches published, omitting such of these latter as represent reconnaissances and preliminary surveys afterwards superseded by more detailed surveys.

LIST OF PUBLISHED COAST SURVEY FINISHED MAPS AND CHARTS.

| Name of Map or Chart. | State. | Scale. | Year of publication. |
|---|---|---|---|
| Richmond's island harbor, | Maine. | 1: 20,000 | 1851 |
| York river and Cape Neddick harbors, | " | 1: 20,000 | 1854 |
| Newburyport harbor, | Mass. | 1: 20,000 | 1855 |
| Gloucester harbor, | " | 1: 20,000 | 1855 |
| Salem harbor, | " | 1: 25,000 | 1855 |
| Boston harbor, | " | 1: 40,000 | 1857 |
| Wellfleet harbor, | " | 1: 50,000 | 1853 |
| Nantucket harbor, | " | 1: 20,000 | 1848 |
| Hyannis harbor, | " | 1: 30,000 | 1850 |
| Harbor of Edgartown, | " | 1: 20,000 | 1848 |
| Harbors of Holmes' Hole and Tarpaulin Cove, | " | 1: 20,000 | 1847 |
| Harbor of New Bedford (edit. in 1846), | " | 1: 40,000 | 1850 |
| General coast from Gay Head to Cape Henlopen, | | 1:400,000 | 1852 |
| Fisher's island sound, | Conn. | 1: 40,000 | 1847 |
| Harbor of New London, | " | 1: 20,000 | 1848 |
| Mouth of Connecticut river, | " | 1: 20,000 | 1853 |
| Harbor of New Haven (edit. in 1846), | " | 1: 30,000 | 1852 |
| Harbors of Black Rock and Bridgeport (edit. in 1848), | " | 1: 20,000 | 1852 |
| Harbors of Sheffield and Cawkins island (edit. in 1848), | " | 1: 20,000 | 1852 |
| Huntington Bay, | New York. | 1: 30,000 | 1849 |
| Oyster bay, or Syosset harbor, | " | 1: 30,000 | 1847 |
| Harbors of Captain's isl'ds, east & west, | " | 1: 20,000 | 1849 |
| Hart and City islands, | " | 1: 20,000 | 1851 |
| Sachem's Head harbor, | " | 1: 10,000 | |
| Hell Gate, | " | 1: 5,000 | 1851 |
| Long Isl. sound, eastern part (edit. 1848) | Conn., R. I. and N. Y. | 1: 80,000 | 1855 |
| " " " middle " " | Conn. and N. Y. | 1: 80,000 | 1855 |
| " " " western " " | Conn. and N. Y. | 1: 80,000 | 1855 |
| New York bay and harbor and the environs (4 sheets), | New York. | 1: 30.000 | 1844 |
| New York bay and harbor and the environs (2 sheets), | " | 1: 30,000 | 1845 |
| N. York bay and harbor and the environs | " | 1: 80,000 | 1845 |
| South coast of Long Island, eastern part, | " | 1: 80,000 | 1857 |
| " " middle " | " | 1: 80,000 | 1857 |
| " " western " | " | 1: 80,000 | 1851 |
| Little Egg harbor, | N. J. | 1: 80,000 | 1846 |
| Delaware bay and river, sheet No. 1, | N. J. and Del. | 1: 80,000 | 1848 |
| " " " No. 2, | N. J. and Del. | 1: 80,000 | 1848 |
| " " " No. 3, | N. J., Del. and Penn. | 1: 80,000 | 1848 |
| Harbor of Annapolis and Severn river, | Md. | 1: 60,000 | 1846 |
| Mouth of Chester river, | " | 1: 40,000 | 1849 |
| Pasquotank river, | N. C. | 1: 60,000 | 1850 |
| Beaufort harbor (on steel), | " | 1: 20,000 | 1857 |
| Charleston harbor (edit. in 1855), | S. C. | 1: 30,000 | 1856 |
| Key West harbor and its approaches, | Fla. | 1: 50,000 | 1855 |
| Mobile bay entrance, | Ala. | 1: 40,000 | 1851 |
| Mobile bay, | " | 1: 80,000 | 1856 |
| Cat and Ship island harbors, | Miss. | 1: 40,000 | 1850 |
| Galveston bay entrance (edit. 1853), | Texas. | 1: 40,000 | 1856 |
| San Diego bay, | Cal. | 1: 40,000 | 1857 |
| Monterey harbor, | " | 1: 40,000 | 1852 |
| Humboldt bay, | " | 1: 30,000 | 1852 |
| Trinidad bay, | " | 1: 20,000 | 1851 |
| Columbia river ent'ce (Alden's survey), | Oregon. | 1: 40,000 | 1854 |

LIST OF PUBLISHED COAST SURVEY PRELIMINARY CHARTS AND HYDROGRAPHIC SKETCHES.

| Name of Chart or Sketch. | State. | Scale. | Year of publication. |
|---|---|---|---|
| Kennebec river entrance, prelim. chart, | Maine. | 1: 30,000 | 1857 |
| Portland harbor, prelim. chart, | " | 1: 20,000 | 1854 |
| " " (for harbor commissioners), | " | 1: 10,000 | 1855 |
| Portsmouth harbor, prelim. chart, | New Hamp. | 1: 20,000 | 1854 |
| Ipswich & Annisquam harb's, prelim. chart, | Mass. | 1: 20,000 | 1855, '57 |
| Gloucester harbor prelim. chart, | " | 1: 20,000 | 1854 |
| Stellwagen's bank, prelim. chart, | " | 1: 400,000 | 1854, '55 |
| Sea-coast of Maine, N. Hampshire, and part of Massachusetts, prelim. chart, | " | 1: 200,000 | 1857 |
| Current-chart of Boston harbor, | " | 1: 100,000 | 1851 |
| Minot's ledge, | " | 1: 10,000 | 1853 |
| Plymouth harbor, prelim. chart, | " | 1: 20,000 | 1857 |
| Provincetown harbor, prelim. chart, | " | 1: 50,000 | 1857 |
| Davis' South shoal, | " | 1: 200,000 | 1846–'53 |
| Nantucket shoals, prelim. chart, | " | 1: 200,000 | 1854 |
| Tidal currents of Nantucket shoals, | " | 1: 300,000 | 1854 |
| Monomoy shoals, prelim. chart, | " | 1: 40,000 | 1854, '56 |
| Muskeget channel, prelim. chart, | " | 1: 60,000 | 1854, '55 |
| Bass river harbor, prelim. chart, | " | 1: 40,000 | 1854, '57 |
| Harbor of Wood's Hole, prelim. chart, | " | 1: 20,000 | 1857 |
| Sow and Pigs reef, | " | 1: 240 | 1853 |
| Sea-coast of Massachusetts, southern part, prelim. chart, | " | 1: 200,000 | 1856, '57 |
| Tidal currents of Long Island sound and approaches, | | 1: 300,000 | 1854 |
| Buttermilk channel, New York harbor, | New York. | 1: 5,000 | 1849 |
| Romer and Flynn's shoals, | " | 1: 40,000 | 1853 |
| Changes in Sandy Hook since 1779, | " | 1: 10,000 | 1851, '53, '55 |
| New York bay and harbor, prelim. chart, | " | 1: 80,000 | 1857 |
| Hudson river, lower sheet, prelim. chart, | " | 1: 60,000 | 1855 |
| " " comparative chart, showing the change since 1852, | " | 1: 20,000 | 1856 |
| Sea-coast of Del., Md., and part of Va., prelim. chart, | | 1: 200,000 | 1852 |
| Delaware and Chesapeake bays and sea-coast, prelim. chart, | | 1: 400,000 | 1855 |
| Chincoteague inlet and shoals in vicinity, | Va. | 1: 40,000 | 1852, '53 |
| Wachapreague, Machipongo, and Metomkin inlets, | " | 1: 40,000 | 1853 |
| Ship and Sand Shoal inlets, prelim. chart, | " | 1: 40,000 | 1854 |
| Chesapeake bay, upper part, 3 sheets prelim. chart, | | 1: 80,000 | 1857 |
| Patapsco river and harbor of Baltimore, prelim. chart, | Md. | 1: 60,000 | 1856 |
| Rappahannock river, 4 sheets, prelim. chart, | Va. | 1: 20,000 | 1856 |
| " " 2 sheets, from entrance to Occupacia creek, prelim. charts, | " | 1: 60,000 | 1857 |
| York river entrance, Va., prelim. chart, | " | 1: 60,000 | 1857 |
| James river (upper sheet) from Richmond to City point, prelim. chart, | " | 1: 40,000 | 1855 |
| Hampton roads & Elizabeth river, pre. chart, | " | 1: 40,000 | 1857 |
| Norfolk harbor, prelim. chart, | " | 1: 10,000 | 1857 |
| Sea-coast of Va., 2 sheets, prelim. chart, | " | 1: 200,000 | 1853, '54 |
| Part of sea-coast of Va. and entrance to Chesapeake bay, prelim. chart, | " | 1: 200,000 | 1855 |
| Gulf-stream explorations, sketch of limits, | | 1:5000,000 | 1853–'55 |
| Albemarle sound, prelim. chart, | N. C. | 1: 200,000 | 1855 |
| Wimble shoals, | " | 1: 80,000 | 1854 |

LIST OF PUBLISHED COAST SURVEY PRELIMINARY CHARTS AND HYDROGRAPHIC SKETCHES.

| Name of Chart or Sketch. | State. | Scale. | Year of publication. |
|---|---|---|---|
| Hatteras shoals, | N. C. | 1: 120,000 | 1850, '51 |
| Hatteras inlet (harbor of refuge), | " | 1: 20,000 | 1849–'52, '57 |
| Ocracoke inlet, | " | 1: 40,000 | 1852, '57 |
| Sea-coast of N. C., from Hatteras to Ocracoke, prelim. chart, | " | 1: 200,000 | 1856, '57 |
| Sea-coast of N. C., from Cape Lookout to Bogue inlet, prelim. chart, | " | 1: 200,000 | 1857 |
| Beaufort harbor, N. C., prelim. chart, | " | 1: 20,000 | 1854 |
| " " " comparative chart, | " | 1: 10,000 | 1857 |
| New river and bar, | " | 1: 15,000 | 1852 |
| Cape Fear river and New inlet entrances, prelim. chart, | " | 1: 40,000 | 1853 |
| Cape Fear river (lower part) from Federal point to Wilmington, N. C., prelim. chart, | " | 1: 80,000 | 1856 |
| Entrances to Cape Fear river (lower part), prelim. chart, | " | 1: 80,000 | 1855, '57 |
| Entrances to Cape Fear river, comparative chart (1851–'56), | " | 1: 20,000 | 1857 |
| Winyah bay and Cape Roman shoals, prelim. chart, | S. C. | 1: 100,000 | 1854 |
| Winyah bay and Georgetown harbor, prelim. chart, | " | 1: 40,000 | 1855 |
| Roman inlet (harbor of refuge), | " | 1: 40,000 | 1857 |
| Bulls' bay (harbor of refuge), | " | 1: 40,000 | 1849, '51, '57 |
| Charleston harbor, Maffitt's channel, comparative chart, | " | 1: 5,000 | 1854, '56 |
| Changes in Charleston bar from 1850 to 1855, | " | 1: 10,000 | 1855 |
| North Edisto river, prelim. chart, | " | 1: 50,000 | 1851, '53, '56 |
| St. Helena sound, prelim. chart, | " | 1: 40,000 | 1857 |
| Port Royal entrance and Beaufort harbor, | " | 1: 60,000 | 1855 |
| Sea-coast of S. C., from Cape Roman to Savannah river, prelim. chart, | " | 1: 200,000 | 1856, '57 |
| Savannah river entrance, | Ga. | 1: 30,000 | 1851 |
| Savannah city approaches, | " | 1: 20,000 | 1851 |
| Savannah river, prelim. chart, | " | 1: 40,000 | 1855 |
| Romerly marshes, | " | 1: 10,000 | 1855 |
| Doboy bar and inlet, | " | 1: 40,000 | 1855 |
| St. Simon's sound and Brunswick harbor, prelim. chart, | " | 1: 40,000 | 1856, '57 |
| St. Andrew's shoals, | " | 1: 60,000 | 1850 |
| St. Mary's bar and Fernandina harbor, prelim. chart, | Fla. | 1: 20,000 | 1856, '57 |
| St. Mary's bar and Fernandina harbor, comparative chart (1843 and 1856), | " | 1: 20,000 | 1856 |
| St. John's river entrance, prelim. chart, | " | 1: 25,000 | 1853 |
| " " " comparative chart, | " | 1: 10,000 | 1857 |
| St. John's river, 2 sheets, prelim. chart, | " | 1: 25,000 | 1856 |
| Mosquito inlet, | " | 1: 40,000 | 1851 |
| Cape Cañaveral shoals, | " | 1: 60,000 | 1850 |
| Florida reefs, from Key Biscayne southward, prelim. chart, | " | 1: 200,000 | 1855, '57 |
| Positions and plan of beacons on Fla. reefs, | " | 1: 400,000 | 1855 |
| Legaré anchorage, prelim. chart, | " | 1: 20,000 | 1855, '57 |
| Turtle harbor, Florida reef, | " | 1: 40,000 | 1854 |
| Coffin's Patches, Florida reef, | " | 1: 20,000 | 1854 |
| Key West harbor and approaches, prel. chart, | " | 1: 100,000 | 1851, '52 |
| Tampa bay, | " | 1: 120,000 | 1855 |
| Waccasassa bay, prelim. chart, | " | 1: 50,000 | 1856 |

LIST OF PUBLISHED COAST SURVEY PRELIMINARY CHARTS AND HYDROGRAPHIC SKETCHES.

| Name of Chart or Sketch. | State. | Scale. | Year of publication. |
|---|---|---|---|
| Channel No. 4, Cedar keys, | Fla. | 1: 30,000 | 1852 |
| Cedar keys and approaches, prelim. chart, | " | 1: 50,000 | 1854, '55 |
| Ocilla river, | " | 1: 20,000 | 1855 |
| St. Mark's bar and channel, | " | 1: 40,000 | 1852 |
| " river, prelim. chart, | " | 1: 80,000 | 1856 |
| St. George's sound entrances, | " | 1: 80,000 | 1853 |
| Appalachicola entrance, prelim. chart, | " | 1: 20,000 | 1857 |
| St. Andrew's bay, prelim. chart, | " | 1: 40,000 | 1855 |
| Pensacola entrance and approaches, prelim. chart, | " | 1: 80,000 | 1857 |
| Sea-coast of part of Ala. and Miss., prelim. chart, | | 1: 200,000 | 1856, '57 |
| Pascagoula river entrance, | Miss. | 1: 20,000 | 1853 |
| Horn island channel, | " | 1: 100,000 | 1846 |
| " " pass, | " | 1: 40,000 | 1852, '53 |
| Biloxi bay, prelim. chart, | " | 1: 40,000 | 1855 |
| Mississippi city harbor, prelim. chart, | " | 1: 40,000 | 1857 |
| Pass Christian harbor, | " | 1: 40,000 | 1851 |
| St. Louis bay and Shieldsboro' harbor, prelim. chart, | " | 1: 30,000 | 1857 |
| Grand island pass and Pearl river entrance, prelim. chart, | " | 1: 40,000 | 1857 |
| Delta of Mississippi, showing changes since 1839, | Louis. | 1: 60,000 | 1851, '52 |
| Lines of deep sea soundings in Gulf of Mexico, with profiles of bottom, | | 1:2400,000 | 1855, '56 |
| Barataria bay entrance, | | 1: 30,000 | 1853 |
| Pass Fourchon, | " | 1: 10,000 | 1854 |
| Timballier bay entrance (Grand pass), | " | 1: 20,000 | 1853 |
| Ship island shoal, near Isle Dernière, prelim. chart, | " | 1: 80,000 | 1853 |
| Vermillion bay & Calcasieu river entrances, | " | 1: 40,000 | 1855 |
| Sabine pass, | | 1: 40,000 | 1853 |
| Galveston bay, prelim. chart, | Texas. | 1: 200,000 | 1851, '52, '55 |
| Sea-coast of Texas, from Galveston southward, prelim. chart, | " | 1: 200,000 | 1856, '57 |
| San Louis pass, prelim. chart, | " | 1: 20,000 | 1853 |
| Matagorda bay entrance, | " | 1: 40,000 | 1857 |
| Aransas pass, | " | 1: 20,000 | 1853 |
| Rio Grande entrance, | " | 1: 20,000 | 1854 |
| Reconnaissance of Western coast, from Monterey to mouth of Columbia river, 1 sheet, | Cal. and Or. | 1:7000,000 | 1850 |
| Reconnaissance of Western coast, from Monterey to mouth of Columbia river, 3 sheets, | " | 1:1000,000 | 1850, '51 |
| Reconnaissance of Western coast, lower sheet, San Diego to San Francisco, | Cal. | 1:1200,000 | 1852, '53 |
| Reconnaissance of Western coast, middle sheet, San Francisco to Umpquah river, | Cal. and Or. | 1:1200,000 | 1854 |
| Reconnaissance of Western coast, upper sheet, Umpquah river to boundary, | Or. & Wash. Ter. | 1:1200,000 | 1855 |
| Cortez bank, off coast of California, | | 1: 10,000 | 1853 |
| " " " " prel. chart, | | 1: 80,000 | 1856 |
| San Clemente island, harbor of refuge at south-east end, | Cal. | 1: 20,000 | 1856 |
| Catalina harbor, | " | 1: 15,000 | 1852 |
| San Pedro anchorage and vicinity of Santa Barbara, | " | 1: 20,000 | 1855 |
| Anacapa island, | " | 1: 20,000 | 1854 |

LIST OF PUBLISHED COAST SURVEY PRELIMINARY CHARTS AND HYDROGRAPHIC SKETCHES.

| Name of Chart or Sketch. | State. | Scale. | Year of publication. |
|---|---|---|---|
| Anacapa and east end of Santa Cruz island, | Cal. | 1: 80,000 | 1857 |
| Prisoner's, Cuyler's & San Clemente harbor, | " | 1: 20,000 | 1852 |
| Santa Barbara mission and vicinity, | " | 1: 20,000 | 1853 |
| " " channel, eastern entrance, prelim. chart, | " | 1: 80,000 | 1857 |
| Santa Cruz, San Simeon, Coxo, and San Louis Obispo harbors, | " | 1: 20,000 | 1852 |
| Point Conception, | " | 1: 40,000 | 1851 |
| Point Pinos, Monterey bay, | " | 1: 20,000 | 1851 |
| Monterey bay, prelim. chart, | " | 1: 60,000 | 1857 |
| Santa Cruz and Año Nuevo harbors, | " | 1: 40,000 | 1854 |
| San Pedro harbor, | " | 1: 30,000 | 1852 |
| San Francisco bay, | " | 1: 400,000 | 1851 |
| Entrance to San Francisco bay, prel. chart, | " | 1: 50,000 | 1856 |
| San Francisco city and vicinity, | " | 1: 10,000 | 1853 |
| South Farallon island, | " | 1: 5,000 | 1855 |
| San Pablo bay, prelim. chart, | " | 1: 50,000 | 1856 |
| Mare island straits, prelim. chart, | " | 1: 30,000 | 1851, '57 |
| San Antonio creek, prelim. chart, | " | 1: 20,000 | 1857 |
| Point Reyes and Drake's bay, | " | 1: 40,000 | 1855 |
| Shelter cove, Mendocino city, & Crescent city harbor, and Port Oxford or Ewing harbor, | Cal. and Or. | 1: 20,000 | 1854 |
| Humboldt bay, | | 1: 30,000 | 1851 |
| Trinidad bay, | | 1: 20,000 | 1851 |
| Umpquah river entrance, | Or. | 1: 20,000 | 1854 |
| Mouth of Columbia river, | " | 1: 200,000 | 1850 |
| " " " | " | 1: 40,000 | 1851 |
| Cape Hancock (or Disappointment), | Wash. Ter. | 1: 20,000 | 1851 |
| Shoalwater bay, | " | 1: 80,000 | 1853, '56 |
| Reconnaissance from Gray's harbor to Admiralty inlet, | " | 1: 600,000 | 1853 |
| Grenville harbor, | " | 1: 20,000 | 1854 |
| Cape Flattery and Neé-ah harbor, | " | 1: 40,000 | 1853 |
| False Dungeness harbor, | " | 1: 30,000 | 1853 |
| New Dungeness harbor, | " | 1: 40,000 | 1856 |
| Canal de Haro, Rosario str'ts, & approaches, | " | 1: 200,000 | 1854 |
| Port Townshend, | " | 1: 40,000 | 1854 |
| Duwamish bay and Seattle harbor, | " | 1: 400,000 | 1854 |
| Smith's or Blunt's island | | 1: 20,000 | 1854 |
| Port Ludlow, | " | 1: 20,000 | 1856 |
| Port Gamble, | " | 1: 20,000 | 1856 |
| Olympia harbor, | " | 1: 20,000 | 1856 |
| Steilacoom harbor, | " | 1: 30,000 | 1856 |
| Bellingham bay, | " | 1: 40,000 | 1856 |
| Blakeley harbor, | " | 1: 10,000 | 1856 |

DISCOVERIES AND DEVELOPMENTS OF THE COAST SURVEY.— Were this Committee to attempt to enumerate or to enter into a detailed specification of all the important contributions to nautical knowledge which the survey has collected, or to point out all the seriously dangerous errors which it has succeeded in correcting, this branch of their subject would overshadow all the rest, and their report would degenerate into a mere catalogue of hydrographic discovery. Yet, in order that the reader may have the opportunity of informing himself, in some measure, in regard to matters which cannot conveniently be fully presented in this place, we shall add a list of the more important discoveries and developments, and if, in connection with this, we compare the ancient charts with those which have been, in later years, wrought out by the American Coast Survey, we may form some faint idea of its inestimable benefits to navigation. Let, for instance, the charts of the port of New York be examined. By far the most serviceable avenue to this most important of our harbors, the channel (Gedney's) made use of now by vessels of the largest class, was entirely unknown until it was developed in the systematic hydrography of the bay. Had the true depth of this channel been known in 1778 (then probably existing, as seen by comparing old and new charts), the French fleet under Count d'Estaing might have passed into the bay, and taken the assembled British vessels. The gradual increase of Sandy Hook to the northward, narrowing the main ship-channel entrance, has been well established by the successive observations of the survey; its cause has been investigated, and the result gives encouragement in reference to the power to control this growth of the Hook, should it become necessary. Take, again, the approach to Philadelphia: until the Coast Survey corrected the error, the two light-houses which define the entrance to the Delaware were variously represented on the charts as from four and a half to seven miles more distant from each other than they are in fact. The great regulating landmark of our pilots, at Bombay Hook, was five miles out of place. The deepest and most

sheltered channel in the widest part of the bay, was supposed to be almost a shoal. Take, again, the island of Nantucket, fringed round with a labyrinth of shoals, which seemed to defy all attempts at comprehension: — see it now all pictured out upon these beautiful charts in every minutest detail, like a great citadel with its outworks systematically disposed all around it. Look at the numerous isolated rocks, ledges, reefs, and shoals, on all parts of our far-extending coast-line, located with the utmost degree of precision.

Some of these individual facts — many or most of them, probably — have already been the means of saving many ships from wreck and total loss. There is no extravagance in the presumption, at least if we strike an average, that a single such service may fairly be ascribable, within the past year, to every one of these additions to the amount of knowledge previously existing in regard to the hidden dangers or the treacherous currents of the ocean. Let this estimate, for a moment, be adopted; and let the value of ships and their cargoes, small and great, richly and poorly laden, be put at so low an average as twenty thousand dollars; it will appear that we may fairly credit to the services of the Coast Survey an annual saving to the country of nearly three millions of dollars. If in this conclusion there is error, there can be little doubt that it is because the amount is put too low.

The recorded disasters to our marine commerce in 1855, reached still a total, in spite of all the increased security which the charts so far as completed have given to navigation, of almost twelve millions of dollars.

In view, therefore, of all these considerations, the Committee cannot but conclude, that, in so far as the direct objects are concerned for which the survey was instituted, the wisdom of the policy in which it originated is abundantly vindicated, and the largest expectations of benefit from its execution which its projectors professed to entertain, have been more than realized in the results already achieved.

*List of the more remarkable discoveries and developments by the United States Coast Survey to the year* 1858, *arranged in geographical order.*

*Casco bay, Maine:* determination of the position of a sunken rock, on which the steamer Daniel Webster struck, on October 13, 1856.

*Gloucester harbor, Massachusetts:* a rock (not on any chart) in the inner harbor, discovered in 1853.

*Boston harbor:* Broad sound channel thoroughly surveyed, 1848; several rocks in the fair channel way, 1854.

*Massachusetts bay:* Stellwagen's bank, an important mark for approaching the entrance, 1854; a dangerous sunken ledge (Davis' ledge) to the eastward of Minot's ledge, 1854; development of a reef between Minot's and Scituate light, 1856; a sunken rock, with only six feet at low water, off Marshfield, 1856; a dangerous rock, near Saquish Head, entrance to Plymouth harbor, 1856; three rocks, partly bare at low water, off Manomet point, determined in position, 1856; a very dangerous rock off Indian Hill, and four miles south of Manomet point, with only six feet water, 1856.

*Nantucket shoals:* Davis' New South shoals discovered, six miles south of the old Nantucket South shoals, and in the track of all vessels between New York and Europe, or from the eastern to the southern States, or to South America, 1846; two new shoals, 1847; six new shoals, the outermost fourteen and a half miles from land, and with only ten feet water, 1848; McBlair's shoals, 1849; Davis' bank, 1848; Fishing Rip, a large shoal, with four and a half fathoms, surveyed in 1852; a ridge connecting Davis' New South shoal and Davis' bank, 1853; a small bank or knoll, beyond the limits of the series of shoals, with only five fathoms on it, five miles east of Great Rip; two shoal spots, near the northern extremity of Davis' bank, with fourteen and eighteen feet water, 1856.

*Nantucket sound:* contraction of the inlet at the north end of

*Monomoy island and opening of new entrance to Chatham harbor*, 1853; *Edwards shoal* south of Nantucket light-boat, 1853; *two coral spots* with twelve and thirteen feet water, 1856; *survey of Muskeget channel* 1845 and 1851.

*Numerous rocks* in Martha's Vineyard sound, Long Island sound, and the various bays and harbors connected therewith.

*The tides and tidal currents* of Long Island sound, of Hudson river, and of the great bay between Massachusetts, Rhode Island, Connecticut, New York, and New Jersey, 1854 to 1857.

*New York bay and harbor:* Gedney's channel into New York bay, having two feet water more than the old channels; changes in New York harbor, between 1845 and 1856; increase of depth in Buttermilk channel, 1845; shoal in main ship channel, 1855.

*Sandy Hook:* its remarkable increase traced from the surveys of the topographical engineers and others, and by several successive special surveys made between 1844 and 1857.

*Delaware bay:* Blake's channel at the entrance, discovered in 1844, open when the eastern channel is closed by ice; Blunt's channel; changes near the Pea Patch.

*Coast of Virginia:* true extent and position of the dangerous shoals near Chincoteague inlet, 1852; Metomkin inlet, shoaling from eleven to eight feet in the channel, 1852; two channels into Wachapreague inlet, both with seven feet water at low tide, 1852; Great Machipongo inlet, a fine wide channel with eleven feet water on the bar at low ebb, and fourteen at high tide, with good anchorage inside in from two to eight fathoms, forming the best harbor between the Delaware and the Chesapeake, 1852.

*Entrance to the Chesapeake:* two shoals, one 4¾ nautical miles S. E. by E. from Smith's island light-house, with seventeen feet water upon it; the other E. by S. nearly 7¾ miles from the same light, with nineteen and a half feet, 1853; find-

ing of only three feet water upon the "Inner Middle," the shoal part of the Middle Ground, west of the "North Channel," at the Chesapeake entrance, 1852.

*Chesapeake bay:* determination of shoals and bars at mouths of Great and Little Choptank, Rappahannock and Patapsco rivers, 1844 to 1855; New Point shoal, with only sixteen feet water, south-east from New Point Comfort light-house, 1854; advantages of York river as a harbor, 1857.

*Gulf stream:* discovery of the cold wall, alternate warm and cold bands, and various other features; discovery of cold water at the bottom of the ocean below the Gulf stream, along the coast of North and South Carolina, Georgia, and Florida, 1853; submarine range of hills, beyond the Gulf stream, tracked from Cape Florida to Cape Lookout, 1855.

*Various facts relative to the distribution of minute shells* on the ocean bottom, of probable use to navigators for recognizing their positions.

*Coast of North Carolina:* dangerous nine feet shoal off Cape Hatteras, 1850; a new channel, with fourteen feet water, into Hatteras inlet, formed during 1852, which is better and straighter than the old channel; changes at Hatteras and Ocracoke inlets, 1857; general permanence in depth on the bar of Beaufort harbor, with the change of position of the channel, 1854; the well ascertained influence of prevailing winds in the movement of the bars at Cape Fear and New Inlet entrances; the shoaling of Cape Fear river bar thoroughly examined for purposes of improvement, 1852, and the changes in the bars and channels determined, 1855 and 1857; Frying Pan shoals, off Cape Fear, extending twenty nautical miles from Bald Head light-house, surveyed in 1851.

*Coast of South Carolina:* Maffit's new channel, Charleston harbor, with the same depth of water as the ship channel, 1850; changes in the channels of Charleston harbor, and in the entrances to North Edisto river and Port Royal harbor, 1852 to 1857.

*Florida reefs:* a harbor of refuge (Turtle harbor) to the

northward and westward of Carysfort light-house, with a depth of water of twenty-six feet at the entrance, 1854; a new passage, with three fathoms water, across the reef, to Legaré harbor, under Triumph reef (lat 25° 30′ N., long. 80° 03′ W.), which, if properly buoyed, will be valuable as a harbor of refuge; a safe rule for crossing the Florida reef near Indian Key, 1854; a new channel into Key West harbor, 1850; Isaac shoal, near Rebecca shoal, not laid down on any chart, 1852.

*Mobile bay:* in 1832, only seventeen feet at low water could be carried over the entrance bar; in 1841, it had nineteen feet, and in 1847, twenty feet and three quarters, as shown by successive surveys; diminution, almost closing, of the passage between Dauphine and Pelican islands, 1853.

*Harbors on the coast of the Gulf of Mexico:* determination of bars, shoals, and passes, and of the effect of wind in disturbing the tides.

*Pacific coast of the United States:* various surveys and charts of small harbors, and a continuous reconnaissance of the entire western coast and islands adjacent a great part of which was imperfectly known, 1849 to 1857.

*Coast of California:* development of a bar at the entrance of San Diego bay, 1856; a shoal in San Diego bay, with only twelve and a half feet water, not on any chart, 1852; determination of the position and soundings on Cortez shoal, and of a point of rock thereon, 1853 and 1856; position of a rock off Point San Pedro, on which the steamer "Uncle Sam" struck, 1855; discovery of red sand in soundings, marking the entrance to the Golden Gate, 1855.

*Columbia river:* south channel surveyed and made available to commerce, 1851; changes of channels, their southward tendency, and a new three fathom channel from Cape Disappointment, due west, to open water, 1852; further changes, 1853.

*Coast of Washington Territory:* shoals and dangerous rocks determined in position, in the Strait of Juan de Fuca, Canal de Haro and Rosario straits, 1853 to 1857

*Additional List for* 1858, *of some of the more remarkable discoveries and developments.*

*Coast of Maine:* dangerous rocks off Boone island, Cape Neddick, and the mouth of York river, determined in position; development of Fishing ledge, Boone island ledge, and Duck island ledge; a detached rock two thirds of a mile northward and eastward of York ledge.

*Coast of New Hampshire:* a very dangerous rock, with only six and a half feet water, off the entrance to Portsmouth harbor, about four nautical miles eastward from the Whale's Back light; a rock, with twelve feet at mean low-water, about four miles and a third eastward of the Whale's Back.

*Coast of Massachusetts:* an extension of the sand spit to the southward of Sunken ledge, Boston harbor, since the survey of 1847.

*Coast of Connecticut:* Luddington rocks, determined in position, about ten yards apart, one mile and a half (nautical) S. W., by compass, from New Haven light-house.

*Coast of New York:* Tidal currents in East river, and surface and sub-currents investigated in New York harbor, the lower bay, and on the bar.

*Charleston harbor:* increase of depth developed in Maffit's channel.

*Coast of Florida:* a new and deeper channel discovered, leading into St. George's sound (Appalachicola), at the eastern end of Dog island, and anchorage connected with it; shoals near the East and West passes of St. George's sound, and a new channel found between St. George's and St. Vincent's islands.

*Coast of California:* Whiting's rock, determined in position, near "The Brothers," at the entrance of San Pablo bay; a reef developed off the Contra Costa flats, San Francisco bay.

*Washington Territory:* a bank of three and a half fathoms,

about a mile off the S. W. point of Sucia island, northern entrance of Washington sound.

AID AFFORDED TO THE LIGHT-HOUSE SYSTEM. — The survey of the coast furnishes also, as part of its direct results, this further and most valuable benefit to the navigator, that it indicates the most appropriate points upon which to establish lights, beacons, buoys, fog-bells, and whatever other constructions may be useful in giving assurance of safety, or indicating the possibility of danger.

For the judicious selection of sites for light-houses, it is necessary to be in possession not only of the hydrography along the coast, but also of the topography of the shore, and of the points which become landmarks in connection with the light-house. This information we have seen that the Coast Survey furnishes, and hence the determination of light-house sites seems to be a part of its appropriate functions. Such has been the view of the subject taken by the government; and accordingly, in 1852, the construction and general disposition of the light-houses of the coast having been confided to a Board composed of officers of elevated rank in the army and navy, with two civilians selected for their high scientific attainments, the examination of suitable sites for these structures was assigned to the superintendent of the Coast Survey. In the six years that have since elapsed, preliminary surveys or examinations have been made in more than one hundred localities on the Atlantic, Gulf and Pacific coasts of the United States, and at least half of the cases reported are based upon recommendations for aids to navigation suggested by the Assistants while surveying in the respective localities.

AID AFFORDED TO THE PLANNING OF THE DEFENCE OF THE COAST. — The bearings of the Coast Survey on subjects connected with the defences of the coast, must be passed over by the committee more briefly than their importance deserves. The eminent soldier at the head of the Engineer Corps of the army, on whom the office of discussing them would have ap-

propriately devolved, was on the eve of departure from the country when the preparation of this report was originally taken in hand. A few paragraphs from his letter to the late chairman, written a short time before his departure, must be accepted as the too succinct expression of his views:

"I cannot," he says, "refrain from urging you to insist emphatically upon the very important aid that has been rendered by the Coast Survey to the system of National Sea-coast Defences, and to the improvement and preservation of harbors. For many years the Engineer Department has had no other resource than the Coast Survey for the general information necessary for the determination of the sites of new fortifications. It has often been without other resources as to local topography and hydrography. It has been enabled, by this aid, greatly to expedite the construction of defensive works of the highest necessity: and I must not omit to add, that the operations of the Coast Survey have, on some occasions, by the kind consideration of the superintendent, been specially arranged in the order of time, and in the manner, calculated soonest to supply our necessities. In relation to the improvement and preservation of harbors, it may be said, generally, that great as the function of exhibiting the coast in its present state may be, a not less important one is the detection of its disturbances: for only on the exact knowledge of these (already found by the labors of the survey to be great and various, and often threatening), can any judicious precautions and remedies be founded. Under the feeling of these obligations to the Coast Survey, I can hardly keep from warming up into specification and amplification — for which, however, I have no time under the arrangements which are pressing on me."

TESTIMONIALS FROM THE MERCANTILE COMMUNITY TO THE VALUE OF THE COAST SURVEY. — Should we aim at conceiving a *definite* estimate of the value of the contributions afforded by the Coast Survey to the development of the commerce, and thereby to the promotion of the general prosperity of our

country, we shall find the importance of these laboriously-gathered results so to grow upon our minds, as we consider them, as to baffle all attempts at computation.

The amount of wealth (and not less of human life) which they have saved from being engulphed in the waves can never be known. The degree to which marine commerce, relieved by them of many of its perils, has been stimulated into higher activity through their influence, must be equally matter of conjecture: so that, in fact, the difficulty of forming any clear or adequate idea of these great benefits, grows, mainly, at last, out of their very magnitude.

To us, whose life is spent upon the land, and whose occupations little familiarize us with the every-day operations of commerce, it is only when our attention is forcibly drawn to this subject, that we feel all its practical importance, or realize the far-reaching consequences of the labors we are reviewing. But it is far otherwise with the merchants and the sea-faring men of the country. Truths which we recognize but occasionally, and by a special effort of the attention, are their daily and familiar thoughts; and thus, whenever an uttered opinion in regard to the operations of the Coast Survey has been heard from such a source, it has invariably been an emphatic testimony to their value.

Thus, the Insurance Companies of *Boston* aver, that "being constantly concerned in the security of the navigation of our own shores, and having watched with interest the condition and progress of the Coast Survey," they "are satisfied that every year it is rendering very valuable service to the commerce of the country by its accurate charts and by its important discoveries;" and they express their conviction that "the present organization of the survey is better suited than any other to insure the prosperity of this great national work, and to provide the best judgment in directing its plans of operation, and the greatest skill and fidelity in their execution."

The Chamber of Commerce of *New York* exult in the "energy peculiar to our country" with which the work has been

prosecuted, "surpassing that of any other country at any time;" and protest that its "continued prosecution is due not only to the interests of our merchants, but as an act of simple though tardy justice to the sea-faring classes of the United States."

The Board of Trade of *Philadelphia* refer to the rapidity and economy with which the survey has been conducted, and to the value of its publications to the merchant, mariner, and underwriter, and declare that they should view its "abandonment, or any limitation upon its present extent and plan, as a public injury."

The underwriters, merchants, and masters of vessels of *Baltimore* speak of the "various discoveries of new channels, new shoals, and other important data on which correct sailing directions are founded," as "of great value in the navigation of our coast and harbors."

The Chamber of Commerce of *Charleston* "consider this work as of more importance to Charleston than any work that the General Government has ever undertaken for it."

The *Savannah* Chamber of Commerce, considering it "matter of great interest to the people of Georgia that the work which has been for some time past going on in the survey of our coast should be prosecuted with vigor," resolved that their senators and representatives be "respectfully requested to aid the grant of the necessary appropriation."

A meeting of citizens of *Mobile* "cordially recommend to the favorable notice and action of Congress the continuation of an energetic and active prosecution of the Coast Survey, as a work of the greatest utility to the various interests of the Union, and calculated to reflect honor upon the intelligence and patriotism of this country," and that their thanks be "tendered to Professor Bache and the officers under his direction, for the zeal and success with which their duties have been performed in the bay and harbor of Mobile."

Still more recently than the before quoted testimonial from New York, appears a memorial signed by the most influential

merchants, ship-owners, and underwriters of that port, who, "believing that the means of safety are increased by the vigorous prosecution of the United States Coast Survey, from its continually developing new dangers, and locating them so that they may be avoided, do respectfully ask that the Secretary of the Treasury may urge upon Congress the necessity of increased appropriations for the purpose of bringing the work to a more speedy completion." Also appears a memorial addressed to the present Honorable Secretary of the Treasury, and signed by underwriters, merchants, traders, manufacturers, mechanics and business men of Philadelphia, who state that they "have for many years witnessed the satisfactory progress of the survey of the coast of the United States," and that "this great work has already proved of eminent advantage to the commerce and navigation of the whole country, by adding to the safety of life and property," and that, "believing that the benefits to be derived from the active and extended prosecution of the Coast Survey are of a character to reflect honor upon the administration which cherishes and supports it, and that we should fail as a people in our duty to the world by neglecting to acquire the most accurate knowledge of the dangers that lie upon our extended line of sea-coast and harbors," they "most respectfully ask the support of the Coast Survey in" the Secretary's "annual report, and the recommendation of ample appropriations for its use."

Sea-coast Changes. — A careful examination of the list of discoveries above given will bring to view a fact which, to a person unfamiliar with this subject, may occasion considerable surprise. More than one in ten of the discoveries of the Coast Survey are discoveries of changes in the ocean's bed; not only among shoals at sea, but also within some of our most frequented harbors, and in the channels by which they are approached. Similar changes occasionally occur, also, even in the configuration of the visible coast line itself. This consideration is sufficient to show that the hydrographic part of this great work cannot, like the geodetic, be, once for all, com-

pleted; but that, in order to maintain permanently in the charts the same degree of accuracy which is originally given to them, the soundings must, from time to time, be revised, and all the changes of depth discovered, carefully noted.

TIDES AND CURRENTS. — Another fact, which the attentive study of the same catalogue discloses, is, that an acquaintance with the ocean currents, and with the irregularities of the tides, is of almost as much importance to the mariner as a knowledge of the depth of the water, or of the form and character of the bottom. These things, and, in addition to them, the magnetic elements, are, in some cases, for the sake of greater clearness, given in separate charts. The present superintendent of the Coast Survey has devoted earnest attention to all the subjects of this class; and he is especially developing the complicated laws which govern the tides on our coast, in a comprehensive and masterly manner. The preliminary chart of cotidal lines on the Atlantic coast, and the tide-tables constructed from the observations of the survey, have been already incorporated into Blunt's "Coast Pilot;" for, although the whole investigation is still in progress, and is necessarily, by its nature, far from its completion, important practical results have been developed which have been immediately put within the reach of the navigator. Preliminary charts of *tidal currents* have also been prepared for various localities at the office of the survey.

Practically valuable, however, as is the investigation of this difficult subject, which, in the present able hands, is proceeding so successfully, it is of the highest interest also in a purely scientific point of view. The problem is one of the few that belong to the globe as a whole; and to us, as possessing a territory which borders on the two great oceans, the solution of the problem appears to be more especially committed than to any other civilized nation.

The subjects here touched upon, sea-coast changes, tides and currents, while, in their practical bearings, as they affect the interests of commerce, they present no aspect which can-

not be equally comprehended by every mind, have yet a higher significancy and stand in a broader relation to the physical history of the globe than is likely to occur to any but those who have made the operations of nature, as they are constantly proceeding on the largest scale over the whole surface of our planet, the subject of long-continued and careful study. The opinions of such men, in regard to the value of observations like those which have just been occupying us, are of much more than ordinary interest; and, on this account, the following, from a member of this committee, whose life has been devoted to labors which make him a high authority upon all matters of this description, is given in his own words:

"The observations on the action of tidal and river agencies in changing the form, depths, and entrances of harbors, are of well-known importance. These changes are so perpetually in progress, and in time become so disastrous to navigation, that a corps charged with the duty of surveying the coast, and mapping out the annual variations of sand-banks, headlands, and channels, is part of the necessary organization of a well-ordered government. But beyond this, the Coast Survey, as at present conducted by Prof. A. D. Bache, is furnishing — in addition to the most exact determination of all the features of harbors, and the special causes of change operating in each, with the progressive variations in the action of these causes themselves — that comprehensive survey of the whole continental coast, and the movements of the bordering ocean, which is essential, in order to arrive at the general principles governing tidal operations and coast changes. It is mapping out the whole grand subject of the Atlantic tides so completely, that the special details of each bay and inlet are taking their true places in the system; and subordinate to these, also, the movements and counter-movements in harbors, which occasion their changes in depth and outline. The questions that must often come up respecting the improvements of harbors, or their protection from encroaching silt or sand, are thus receiv-

ing thorough solution by reference to the widest and most fundamental principles."

The committee here perceive that they are approaching the limit at which the practical becomes blended with the scientific; and while those results of the survey which remain to be examined are by no means deficient in value, nor often even in a value eminently practical, yet, since they cannot properly be classed among the explicit objects in view of which the work was originally instituted, their consideration appears most fittingly to fall under the head of *Collateral Results.*

## COLLATERAL RESULTS. — ADDITIONS TO THE AMOUNT OF SCIENTIFIC OR USEFUL KNOWLEDGE.

CONTRIBUTIONS TO GEOLOGICAL KNOWLEDGE. — All observation conspires to instruct us that the surface of our earth, under the action of the elements, is undergoing continual changes; and that the relation existing between land and sea is in a state of incessant oscillation. What the Coast Survey has disclosed to us, by its severely exact methods of scrutiny, of the mutual and steadily progressive encroachments of the water and the land, along the entire extent of our own ocean border; that, also, successive generations of men, observing without method, have, in the lapse of centuries, been compelled to recognize, as a law of universal prevalence, wherever, throughout the globe, these contending elements come into contact; and that, too, many facts of recorded history attest, in instances in which the tooth of time has so completely obliterated all natural traces of the former state of things, that, but for the existence of the record, not a suspicion of change could ever have been entertained. That, also, many imposing monuments of ancient art, colossal mementoes of forgotten centuries, once towering sublimely above the pigmy dwellings of their builders, but now half-buried in encroaching sand, avouch in solemn silence to the curious wanderer of the desert: while other ruins, no less interesting, tottering on the margin

of the sea, reveal the same truth in the deep-worn evidences, which they bear far up among their crumbling sculptures, of former watery violence; and in the still more wasting ravages of the lithodomous molluscs which burrowed in their substance when the waters, which vainly lash their foundations to-day, enveloped them, in some unknown period of the past, half-way up to their elaborately wrought capitals. That, also, islands upheaved above the ocean's surface within the period of authentic history, or cities, within the same period, submerged beneath its waves — extended lines of coast which the paroxysmal throes of volcanic energy have suddenly elevated many feet above their former level, or as suddenly depressed below, — or which the steady and long-continued progress of secular change has lifted or submerged more slowly; that, also, the growth or wasting of the shores under our own eyes, in numberless localities where no change of the general level is perceptible; that, also, the rounded and water-worn forms of the pebbles which lie imbedded, in countless multitude, in the superficial strata of every continent, — and the vast deposits of marine organisms which crown the very mountain-tops with evidences of once swarming primeval life; that, in short, an almost endless series of unequivocal and most convincing indications proclaim, in modes as various as the mute witnesses themselves, but with a harmony of testimony as irresistible as it is impressive.

Considerations like these invest the researches of the Coast Survey with that species of fascinating interest which attaches to mystery, and impart to them a portion of the dignity which belongs to those grand operations of nature, whose laws they assist us to unveil. This noble work is gathering for us the statistics of secular progress; and it is no idle imagining to suppose that, out of its systematized results, and those of other equally searching investigations already in progress, or hereafter to be instituted in other lands, science may yet be able to unfold to us all the wonderful history of those stupendous revolutions which have marked the past, and all the inev-

itable transformations which still impend over the unknown future of our globe. Upon the point here presented, the committee take the liberty to quote once more the language of the eminent authority already introduced.

"Geology is gathering much from the results of the Coast Survey. The rocks of the globe have been formed, to a great extent, through tidal and current action, many of them having been accumulated, just as sand and mud deposits are now accumulating along and off the shore from New Jersey south, or as deltas are forming about the mouths of rivers. The operations of the present day thus explain those of the earth's past history. Science is hence deriving from the researches of the Coast Survey, facts elucidating the whole series of geological formations. The surveys of the delta and mouths of the Mississippi, of the harbors of Mobile and Charleston, of Delaware river and bay, of New York and other bays and harbors, are all subjects for profound study with those who would understand this great branch of geological dynamics; and they look for other important data from the future surveys, that shall exhibit the changes that are now in progress, and the rate at which they are going forward.

"Besides investigating the origin of sea-shore deposits, and the causes of their varying features, soundings off the coast and in the deeper waters are making the sea-bottom a part of *terra cognita*, disclosing the positions of its valleys and plains and the character of its slopes, and marking out the true limit between the continental areas and the great oceanic depressions. The Coast Survey is thus enabling the geologist to extend the area of research, compare the causes and characters of submarine ridges with those of the sub-aerial, and trace the directions of river channels over the sea-bottom off their mouths.

"By its registers of depth, it is also preparing to detect evidences of variation of level along the border of the continent; or evidences of stability, as the case may be: and this is becoming a subject of practical importance. Suspicions have

been thrown out respecting a slowly-progressive subsidence along the coast of New Jersey, which, if sustained by a thorough examination of the facts, suggest a new topic for serious consideration to all interested in New York commerce. Whether true or not, the point requires examination for every part of the coast of the United States; and this examination it has begun to have, in the accurate system of soundings and measurements of the Coast Survey.

"Along the coast of southern Florida, the subject of coral reefs comes under the attention of the survey; and in investigating the forms and formation of coral-reef harbors, like that of Key West, their rate of filling up, or the influence of currents in keeping them open, geology is receiving other important contributions from the facts which its operations are disclosing.

"It may finally be observed, that the study of the material brought up by the lead, in the prosecution of the deep-sea soundings, is making some remarkable revelations with regard to the distribution of animal life; and that thus zoölogy and the geography of life are deriving much from the investigations in progress.

"All these scientific contributions are collateral results, incidental to the great purpose of the survey — results attained, not through independent researches needlessly introduced, but because they are of necessity involved in the prosecution of the great purpose itself, and thus constitute a part of the evidence demonstrating the strict fidelity and profound ability with which it is carried out. They flow from the work as its entirely legitimate fruits, and take their place, of unquestionable right, along-side of those other fruits whose legitimacy is universally recognized, and in which humanity, as well as commerce, is so deeply concerned."

As somewhat more specifically illustrative of the nature of the interesting inquiries connected with this subject, which the Coast Survey is solving, or will yet be able to solve, the committee venture to quote, in addition to the foregoing, the sub-

joined passage from a communication obligingly furnished by another geologist, standing deservedly high among the scientific men of the country:

"The Mississippi river," observes this gentleman, "presents, perhaps, the most magnificent example in the world of a large river discharging abundant sediment into an almost tideless sea. In the Gulf of Mexico, therefore, we have the best opportunities of studying the phenomena of river deposit. The distribution of the various kinds of sediment brought down by the great river, the slope of the submarine bank, destined to be gradually transformed into delta and added to our territory — these would be problems worthy, in themselves alone, of the attention of an enlightened government. And these important problems have not escaped the attention of the accomplished superintendent; as may be seen in the report for 1856, map No. 40, where the distribution of sand, mud and coral-mud is carefully laid down. But there are several points of interest which deserve further attention: It will be seen, by reference to the map just mentioned, that the lines of equal depth approach each other near the mouth of the Mississippi, showing that the submarine bank formed by the deposit of this river has a very rapid slope toward the south. Near the mouth of the river runs the line of six fathoms; about ten miles south, the line of fifty fathoms; and from this point the bottom slopes so rapidly that, at the distance of three or four miles more, a line of two hundred and thirty-six fathoms found no bottom, making a difference of nearly eleven hundred feet in this short distance. Thus we have *a considerable inclination of strata* over large areas, produced by the *natural agency of water alone*. It is possible, however, that the steepness of the slope may be, in part, due to the existence of currents (*e. g.* Gulf-stream), sweeping across the mouth of this river at some distance, and carrying away sediment which would otherwise be deposited further out. It is important to ascertain how far this southward slope is modified by the existence of any such current.

"The same map shows that, on the Gulf side of the peninsula of Florida, the coral bottom (probably coral mud) extends as far north as Tampa bay. It would be extremely interesting to determine whether there is any return current carrying coral mud from the southern point and keys of Florida, northward, or whether, as is much more probable, we have here additional evidence of the fact that the coral formation of the peninsula extends northward as far as Tampa. It will be recollected that, according to Tuomey, the *eocene* may be traced as far south as this point. It would be important to trace, in a similar manner, the coral deposit on the east side of the peninsula as far as possible."

Were it thought indispensable in this report to pursue a method severely logical, it would be proper here to complete whatever remains to be said in regard to the aid which the survey of the coast is rendering to geological science. Inasmuch, however, as the formation of submarine strata depends, as we have seen, in great measure upon the distribution of sedimentary matter through the agency of currents, and inasmuch as the greatest of all the currents in our waters, or indeed in the waters of any known sea, has been a subject of special study and elaborate exploration, under the direction of the superintendent of the Coast Survey, it has seemed convenient to defer the consideration of the relations of that great stream to geology until some account shall have been given of the character of the observations made upon the stream itself, and of the results which those observations have gathered.

EXPLORATION OF THE GULF STREAM.—The observations made in the progress of the survey of the coast, with regard to the currents of the ocean, and particularly with regard to the Gulf stream and Labrador current, are in point of importance not inferior to those made on the tides. The oceanic currents embrace completely the whole globe, and follow as regularly an established system in their movements as the blood circulating in a living being. This, therefore, in its most general aspects, is one of those world-wide subjects which nations

should undertake to investigate; but, in the actual discharge of the duty thus resting on them, it is fitting that each nation should explore, with special attention, the seas that wash its own shores. To ourselves, whatever relates to the Gulf stream possesses a paramount interest, since every vessel which approaches our Atlantic coast from abroad is compelled to cross this great river in the sea. In the exploration of the stream which the Coast Survey has undertaken, the velocity and direction of the current at all points are, of course, primarily noted; but especially is the temperature of the water made a subject of very careful study in all parts of the stream. The distribution of temperature in the axis of the stream, at different depths, and in sections at right angles to the axis, has been diligently investigated, and is exhibited, so far as it has been hitherto ascertained, in preliminary charts. The investigation embraces, also, the *position* of the axis of the stream at different seasons of the year, with the variations in its breadth, the changes in the distribution of temperatures at different seasons, and the connection of the figure of the bottom with this distribution; also, the characteristics of the inner cold current making in a direction opposite to that of the Gulf stream; all of which points are materially interesting, not only to general science, but to practical navigation. With the aid of the perfected map of this stream, and of its attendant counter-current, such as the survey will eventually bring forth from its accumulating materials, the navigator approaching our coast will obtain most important indications of his position. The color of the water itself will be one of these; but a more interesting one will be found by merely consulting the temperature of the sea, extending the observations to different depths below the surface, as well as noting the superficial temperature. Thus the remarkable and sudden change of temperature which occurs along the inner edge of the current, the warm water of the Gulf stream being confined as it were by a "cold wall" of water (a term happily applied to it by Lieut. G. M. Bache) is a valuable practical guide; yet, in order that it may be a safe

one, the laws which govern the position of the "cold wall" at different seasons, must yet be carefully determined. This, and various matters of interest still unsettled or obscure, can be properly developed only by a series of observations, extending over a number of years. The Coast Survey is, in fact, now supplying what Humboldt, so long ago as the year 1804, clearly set forth as important desiderata in relation to the Gulf stream — a subject to which he had devoted much careful attention. [Personal Narrative, &c., chap. i.]

We may, at this point, with propriety resume the subject, deferred in the discussion of the topic last considered, of the relations of the Gulf stream to the submarine deposits heretofore formed, or still in process of formation, upon our shores and beneath the waters of the neighboring seas. The formation of shore deposits, whether in tideless or in tidal seas, has been investigated to some extent elsewhere, particularly under the direction of the English government, in the British seas and the German ocean; but of the deposits of oceanic currents in deep seas, we know nothing but what has been revealed by the Coast Survey.

One of the most important contributions by which American geology has ever been enriched, is the discovery made by Agassiz, under the direction of the survey, that the peninsula of Florida is of recent origin; that it has been formed, in fact, by the growth of successive coral reefs, one outside of the other, from north to south; and that this progressive growth is still slowly proceeding. It is, however, the plausible suggestion of Prof. Joseph Le Conte, that, in order to furnish a foundation on which the corals might build, it is necessary that some previously acting cause should be taken into the account; inasmuch as probabilities favor the supposition that the water in which the present reefs are found has been, at a former period, too deep for the operations of these polyps. Such a cause he finds in the great stream we are considering. The waters of this stream, emerging from the Gulf of Mexico highly charged with sediment, and sweeping in a curve around

the point of Florida, must, in his view, according to the laws of current action, have deposited a portion of their suspended matter within the curve. And thus, as he supposes, a submarine bank has been progressively formed, extending slowly southward, upon which the distinct reefs pointed out by Agassiz have been successively built. Whether or not this theoretic view is correct, is a point in regard to which geologists will look with interest to the investigations of the Coast Survey to determine.

Another interesting class of facts, associating itself naturally with the explorations of the survey, is that which relates to the agency of the Gulf stream in determining deposits in the open ocean. The velocity of this powerful current, as it rushes through the straits of Florida, is not less than five miles an hour; and with this great velocity it emerges to the northward into a wider sea. It is a property of currents, in such cases, to form an eddy on either side: and if the stream bear sediment, a bank is the necessary consequence. A notable instance of this is seen in the tidal current which rushes through the British channel into the German ocean, forming eddies which have resulted in the production of submarine banks. The Bahama banks lie east of Florida, separated from that peninsula by the Gulf stream, and just in the position in which the existence of the stream would naturally lead us to look for such deposits. Upon these banks, also, coral reefs and keys have been formed. It is highly desirable that they should be further examined with reference to this point, and it is to be hoped that the operations of the survey may be so extended as to cover them.

The very remarkable configuration of the sea-bottom in and about the Gulf stream, which has been so beautifully brought out by means of sections in the Coast Survey report of 1853 (maps No. 15 and 16) is a fact of great importance in a geological point of view. Commencing from the coast, the sea-bottom deepens very gradually, so that, at the distance of fifty miles, only the depth of twenty fathoms is attained. From

this point, the bottom slopes rapidly down to very great depths. Thus the Gulf stream runs like a river which has overflowed its banks. Now this submarine river-bank exists all along the coast side of the Gulf stream. It is upon the margin of this bank that the coral reefs of Florida have probably grown. The bank may have been produced everywhere, by deposit from the Gulf stream; or it may be true that its formation has been the result of sediment from all the Atlantic rivers, deposited for a certain distance off shore, and then, as it were, cut off and carried away by the current of the stream. Both causes have, not improbably, been concerned in producing the observed effect; but the question is one which cannot be regarded as settled, and one on which the Coast Survey will yet probably shed much additional light.

The sections already spoken of, in addition to the lateral bank just mentioned, reveal the existence of enormous ridges and correspondingly profound hollows, running in the direction of the stream, and therefore nearly parallel to the coast. The stream is, moreover, divided longitudinally into several alternating bands of warm and cold water; the bands of warm water corresponding to the hollows, and those of cold water to the ridges. These bands and corresponding hollows and ridges commence at Cape Cañaveral, and extend to a great distance; becoming, however, more indistinct as we go further northward. One of the cold bands runs along the lateral bank, and is that which has been already mentioned under the name of the "cold wall" of the Gulf stream. In this, a slight return current has been detected; but whether or not the current is permanent, is a question yet to be definitely settled.

Now the origin or occasion of these cold bands is a question of the highest interest. It may be inquired, Are they bands of Atlantic water, alternating with the water of the Gulf, or are they the cold water of a return polar current, deep-seated generally, but rising somewhat nearer the surface along these lines? It is very important, in view of these questions, that the *direction* of the current of the cold bands should be determined,

and, if the warm and cold waters tend in the same direction, that their *relative* velocity should be ascertained.

It is, again, a question of no less interest than the foregoing to what cause we are to suppose the alternating ridges and hollows to be due. Possibly they are mountain ranges, with their corresponding longitudinal valleys; constituting, thus, a submarine system parallel to the Appalachian chain. Possibly they are only lines of sedimentary deposit, determined by the existence of the cold bands of comparatively still water, or perhaps even of return currents. If the latter supposition be correct, then the ridges should decrease in height as we go north; for the sedimentary matter must sink, as it is borne further, to a lower level. And even if the former be the true supposition — that is to say, if these elevated ridges are mountain chains of igneous origin — it is still possible that they may have undergone great modifications of form, from the subsidence upon them of the matters held in suspension by the waters of the Gulf stream.

The currents of the ocean suggest, by natural association, the movements of the more subtle medium above it — movements no less important to the navigator than those of the ocean itself, since the question of his safety is often dependent upon the degree to which they may be favorable or adverse. On this subject the Coast Survey has already accumulated much valuable information; and in its further progress it will unquestionably gather much more. It is, therefore, one which may appropriately claim the next place in this review.

CONTRIBUTIONS TO METEOROLOGICAL KNOWLEDGE. — It is obvious that a correct knowledge of the force and prevailing direction of the winds in different seasons of the year, especially in the vicinity of harbors, must be of the highest importance, not only to the safety of vessels and their cargoes, but to the security of human life. The physical causes which give rise to the great general atmospheric currents may be considered as satisfactorily established; but the influence of local causes in modifying these currents is yet very imperfectly un-

derstood. It is well known that the direction of mountain chains and river valleys frequently determines the course of the prevailing winds. This is strikingly exhibited in various places on this continent, and especially in the valley of the Hudson and Mohawk. And the analogous conditions which every line of coast presents, lead to the necessary conclusion that such modifications of atmospheric currents must be produced by them *near the land*, which must either be very serviceable or very dangerous to the navigator, according to the state of his information.

Observations bearing on this question have been incidentally made, in connection with the tidal observations of the Coast Survey, under the judicious guidance of the superintendent; and these are sufficient to throw some light on the laws which regulate the winds at certain points along our coast. At Fort Morgan, at Mobile point, and at Cat island light-house, near the entrance to Lake Borgne, the observations have given evidence of local peculiarities, at once interesting to science and important to navigation, distinguishing the winds which prevail in this part of the Gulf of Mexico. The remarkable frequency of north winds at this point, and the absence of west winds, so strikingly brought out in the graphic delineations of the superintendent, are probably in a measure due to the influence of land.

The effect of the winds, according to their varying force and direction, upon the level of the water in harbors and inlets, is another interesting result incidentally developed by the survey, in connection with the investigation of the tides. The discussion of the observations made during the hydrographic survey of Albemarle sound, clearly indicates the influence of the wind on the height of the water, and has furnished the means of tracing several anomalous effects up to their true causes. A similar discussion of the influence of the wind on the tides in Cat island harbor on the Gulf coast, leads to the same general conclusions, with differences in the numerical details, which are of high scientific interest. The observations con-

ducted for some years past at Key West, Fort Morgan, and Galveston, and a careful investigation of the winds of the Gulf coast, made for purposes of navigation, and as determining the working season of the Coast Survey, have already led the superintendent to many valuable deductions; and it is highly desirable that similar observations should be made at every point commercially prominent along our ocean border. Such researches have an obvious bearing on the safety of navigation; and those which relate to the effects of winds on the waters of harbors and inlets are absolutely indispensable, in order to secure the necessary accuracy in the hydrography of the coast.

Other questions connected with atmospheric influences which the survey may yet clear up, can be but hinted at. Such are, for instance, the effects of rain or drought upon the height of the water in those harbors, inlets and bays which receive the waters of extensive hydrographic basins, and the inquiry, how far the mean level of the ocean is affected at any given place by the pressure of the atmosphere upon its surface.

CONTRIBUTIONS TO OUR KNOWLEDGE OF TERRESTRIAL MAGNETISM. — Ever since the directive property of the magnetic needle became known, that little instrument has been regarded as an indispensable part of the outfit of every vessel navigating the seas. Its utility to the voyager is of so universal notoriety, that no argument is necessary to prove the value of any species of research whose result is either to add to the sensitiveness of the instrument itself, or to perfect that knowledge of the earth's magnetic influences which is essential in order that its indications may be practically truthful or trustworthy. The use of the compass is to denote to the mariner the position of his meridian; but the needle itself very rarely coincides in direction with the meridian. What, therefore, the mariner must know, if he would not be misled by the very guide in whose infallibility he often for days together implicitly trusts, is the exact amount, in angular measurement, by which the visible indication of his instrument is fallacious.

Now, the magnetic meridian is, of course, laid down upon the charts. But this meridian is not fixed. Its position on the chart is true only for the time when the chart is constructed; or rather, for the time when the observations upon which it rests are made. It is therefore an important problem to determine, if possible, by what *law of change* the position of this meridian is affected, at every point of the coast. But it is entirely evident that this can be done only by comparing observations regularly made during a long period. It can be done, in short, in no other way but by that species of patient, and laborious, and protracted observation, carried on simultaneously at a large number of scattered stations, which, in the earlier part of this report, has been described as the method which the superintendent of the Coast Survey, with all that ability and knowledge of terrestrial magnetism for which he is distinguished, is actually pursuing. These observations are not, of necessity, confined to the immediate coast. It is, on the contrary, desirable, and even requisite, that they should be extended into the interior of the country, in order to develop the curves of equal declination (*isogonic lines*), or those, in other words, upon which the variation of the compass is the same. The determination of these curves by observation at sea, with the precision requisite for the development of the laws of change, is almost if not quite impossible; but from the curves on land, extended beyond the coast, all that can be desired will be finally attained. What has been thus far done is exhibited in valuable preliminary charts. It is hardly necessary to observe that the collateral investigation of the magnetic inclination, or dip, and of the intensity of the magnetic force, constituting, indeed, an essential rather than a collateral part of any investigation relating to terrestrial magnetism, have been prosecuted *pari passu* with that of the declination.

The results thus gathered possess a double value; for while, to the mariner, they associate themselves with questions which involve his personal safety every day and every hour, to the philosopher who devotes himself to the study of nature in her

mysteriously varied manifestations of force, they possess a curious interest which grows with the aliment it gathers, and sustains his zeal in quest of clearer light, under all the oppressive labor of observations multiplied without limit, and of reductions indescribably tedious.

But there is another species of importance attaching to the results elicited by the investigations we are considering, which recommends them to the attention and favor of citizens of every class. It has already been intimated that the direction of the magnetic needle is not only in general not due north, but that it is widely diverse in different localities, and never constant in the same locality. Near our extreme north-eastern boundary, for example, the deviation is at this time fifteen degrees to the west of north, while on the borders of the Pacific ocean it is no less than twenty-one degrees to the east of north. Between these limits it has every intermediate value. Now, were this state of things permanent, we might accommodate ourselves to the diversity, wide as it is; and when once the actual variation had been ascertained for every locality, no further inconvenience need be experienced. In the early history of this science, and before the fact had been detected to what an extent the declination of the needle is subject to change, it was regarded by the British government as a matter of so high importance to ascertain the true amount of the declination in every part of the world, that the celebrated Halley was sent out in command of a government vessel, to circumnavigate the globe at the public expense, in order that he might gather materials for the construction of a magnetic map of the world. His mission was faithfully accomplished, and the required work was executed in a manner worthy of all praise; but hardly had his map been given to the world before the steadily progressive natural changes of the earth's magnetic forces had vitiated all its indications, and rendered it, for the purposes intended, practically useless.

Now the manner in which these changes affect landsmen will require no explanation, when it is recalled to memory that

all the boundaries of landed property are run out by the help of the magnetic needle. But the line which bore due north, by compass, or bore in any other specific direction, twenty years ago, or ten years, or even one, varies from that direction at present very considerably. When, therefore, a not very well informed surveyor (and there are a good many such in the country) attempts to use these former bearings to fix the line-fences of a farm, he runs out a *new* piece of land, of precisely the same size and shape as the original tract, but with no single one of its boundaries coinciding with the original lines; so that the new piece of ground thus laid out overlaps the neighboring farm on the one side, and leaves a gore between it and the neighbor on the other. Then comes litigation in its most perplexing form. But these embarrassments will be removed, for all future time, from all places included within the range to which the magnetic observations of the Coast Survey may extend. The survey not only gives the actual variation at definite dates, but, by a beautiful application of mathematical principles, involving most abstruse considerations, it has deduced the *law* which governs the changes, and made it possible accurately to predict their future, and to reveal their past.

The Coast Survey reports for 1855 and 1856 contain tables of magnetic observations and charts of the magnetic declinations, dip and intensity, in which the information thus far collected is presented in very perspicuous form. Discussions of the law of secular variation, with diagrams illustrative of the same, are also given there.

The *true bearings* between the Coast Survey stations being known, and published in the lists of Geographical positions (C. S. reports for 1851–53–55–57), it is a very simple matter for a surveyor, living in the vicinity of such stations, to ascertain the variation of his compass at any time by observing the *compass bearing* of a line between such stations. In many instances the assistants in the Coast Survey have also planted *meridian marks*, at the request of local authorities, to facilitate the ascertainment of the variation of the needle.

BEARING ON GEODETIC SURVEYS OF THE STATES. — It has been seen that the ordinary methods of surveying are unsuited to the work of delineating the boundaries and territories of a State. In fact, that which is called a State map in our country is rarely more than a forced combination of county maps, these made up in turn from the surveys of townships, or else determined by arbitrary guess-work. It is obvious that the townships, however accurately surveyed by compass and chain, can never be represented truly on the composite map of a country; and that the maps of counties so made up cannot be brought together so as to exhibit a correct view of the State. If the surveyor has assumed a southern angle as his point of departure, each northern line will be too long; and if his survey has begun at the northern extremity, all his southern lines will be too short. Errors similar in their result will attend the running of eastern and western boundaries; and at last, with all these errors, both of latitude and longitude, the lines of the survey must be cut down or extended, to bring them into some semblance of relation with the equally erroneous lines of the divisions which are to surround it on the map.

It is within the knowledge of almost every land-surveyor in the older States, that the surveys under which titles were made from the first proprietors almost invariably overlap each other, or leave vacant gores and quadrangles between them. Nor has this wholly resulted from the changing variation of the compass, though we have seen that that cause may easily produce the effect; but from fundamental error in the principle on which common field surveying rests — error which rapidly opens out, as the area surveyed is enlarged. So notorious and general are, or have been, these conflicts of title, that courts have long since held that wherever natural, or even artificial landmarks can be found, the angles and lines of the record are to give place to them. Let any one compare the modern trace of a railroad with a map of the State which it traverses, and he will find that neither the bearings nor the distances agree. Let him undertake to measure the farms which are contained

within any one of the measured triangles of the Coast Survey, or, if he prefer it, within the local map of his own township, and he must not be surprised to find — for he assuredly will — that the whole is either greater or less than the sum of the parts.

In older countries more than in our own, yet quite enough even among us, this has been recognized as a serious evil, embarrassing titles, inviting litigation, and, in so far as such causes are operative, discouraging and impeding agricultural progress. The result abroad has been, that the government has assumed the burden of ascertaining for every man the true boundaries of his landed possessions. The geodetic map of France marks down the lines of every farm as definitely as it does the site of a light-house, or the course of a stream. The ordnance maps of England are some of them on a minor scale; but their tracings are almost equally minute: while the more recent surveys of Scotland and Ireland are expanded to the proportion of an inch to the acre, and note even the minor subdivisions of fields and messuages — a scale not too large, where the revenues of government are derived in part from a tax on lands.

Should it be determined hereafter, by any of the States of our Union, to frame an accurate map of its territory, the Coast Survey will have furnished many of the indispensable elements. It has determined for us the absolute position on the earth's surface of at least some of the most noted points of our domain; and of these several are in the interior of the country. Such determinations, far from the ocean, may appear at first sight to have no natural connection with a coast survey. The necessity of longitude determinations on the coast itself, is obvious enough to any one; not only because they are an essential element of the geodesy, but because they afford the mariner convenient opportunities of verifying the sea-rates of his chronometers, by comparing the well-determined longitudes of these points with those which the chronometers give when he visits them. Neither of these objects

seems to be subserved by longitude determinations in the heart of the country; but when we recall the extent to which the telegraph has been of late years called into use, in determining differences of longitude between distant points in our country, it will be seen that it is a question of secondary importance precisely *where* those places are situated whose longitudes from Greenwich are first accurately settled, in order that they may be used as standards of comparison for the points on the coast with which they may be in electric communication: and accordingly the points first and most naturally selected will be those, no matter where, at which facilities already exist for making accurate astronomical observation. Such points, for example, are Hudson and Cincinnati, Ohio, both of which places are provided with permanent observatories. Another relation, also, these interior longitude determinations have to the coast survey. With the march of the telegraph they will be pushed forward to the west, and at no distant day they will form a grand chain extending across the continent, serving in a most satisfactory manner to verify the independently determined longitudes of the Pacific coast.

For the present, however, our interest in these determinations is found in the assistance they will render to any triangulation which may be set on foot, for the construction of an accurate state map. The number of them which the coast survey has made is considerable, and will be greater as the necessity or convenience of the survey may require; but as every such determination is attended with some expense, they are not of course multiplied, except on such considerations. The existence of the work, however, affords our legislatures a very favorable opportunity, if they will avail themselves of it, by the exercise of a moderate liberality, to secure the exact latitudes and longitudes of all their capitals, and all their principal cities.

Another facility toward the institution of geodetic surveys of the States may be found in the numerous perfectly ascertained base lines which the survey has measured, and which

may be used as the foundation of a local triangulation extending into the interior of each State from the ocean; till at length the Atlantic and Pacific surveys may verify each other in the axis of the continent.

But more than this, in the work as prosecuted along the coast, a *system* has been introduced and perfected; and in any future surveys, which may be undertaken, no part of the processes will be tentative or doubtful. The work of State surveys could therefore be done with the highest expedition and the least expense compatible with the nature of the undertaking.

And finally, under the auspices and the instruction of the present energetic superintendent, there have been training up a body of highly educated, active, efficient and disciplined men, zealous in their profession, and familiar with every detail of field work, computation and record, that can enter into the geodesy of a State.

All this the survey has already done in the way of facilitating the execution, by the action of the individual States themselves, of a work so greatly on many accounts to be desired, as a geodetic survey of the whole country. But there is one work more which it seems almost indispensable that it should yet do, by way of securing the highest perfection of accuracy to its own results; and which would greatly increase the facilities we have been considering, while it would at the same time extend them into a number of the interior States beyond the limits of its present operations. The work here intended is a triangulation, in a direct line from Maine to Alabama, striding along the Appalachian chain, the back-bone of the country, executed for the purpose of checking and verifying the seaboard triangulation. The facilities for large triangles afforded by the elevated summits of this mountain system, would make it practicable to span the whole distance by a very few steps, involving a far less liability to accumulated error than the triangulation along our circuitous coast; of which a large portion, low and densely wooded, excludes the possibility of long lines of sight. This air line triangulation,

measuring the chord of the great arc formed by our Atlantic and Gulf coast, is, in the opinion of this Committee, so eminently desirable, that they cannot but express the hope that it may receive the sanction of Congress at a very early day. It would furnish, directly, base-lines for the geodetic surveys of a great number of States, some of which do not yet possess any thing deserving the name of a map of their territory. This is the case with all of the original States, Massachusetts excepted; these not having been, like the younger members of the confederacy, mapped by the system of public land surveys. The maps constructed upon the data furnished by the latter, are, indeed, in a great degree illusory; and they must eventually be superseded by proper surveys: but the errors and contradictions found in attempting to construct a map of one of the older States from existing local surveys, are enormous, and admit of no accommodation.

It is indeed gravely to be regretted that the surveys of our public lands are not conducted by the methods of geodesy. The system now in use found its apology at first in the want of scientific familiarity with the subject in this country. Its essential errors are only beginning to unfold themselves in the experience of the settlers; but they are errors which, from their very nature, must go on enlarging themselves in accumulating ratio. A system of triangulation would have made the nominal limits and areas correspond with the actual; and would, at the same time, have constructed a series of maps of the new States without cost, and absolutely accurate.

The belief is held by some among us that it is not the office of the Federal government to institute a survey of the entire domain of the nation. Such convictions the Committee can respect, though they can by no means unanimously partake them. But whatever variety of opinion may be entertained on this point, surely no one will regret that, in the exercise of a power which is unquestioned in any quarter, the government of the Union has stimulated the governments of the several States to undertake a work in the exercise of their own sover-

eignty. It is surely well that operations which, like those of the Coast Survey, are made imperative by the exigencies of commerce and of a general system of defence against foreign invasion, should also proffer benefits to those States which do not share directly in the hazards of navigation, nor need fortresses to protect them against the approach of an enemy.

BEARING ON WORKS OF INTERNAL IMPROVEMENT. — In the earlier part of this report, a brief description is given of the field operations by which the topographical features of the land bordering the ocean are ascertained and delineated. It was there stated that the lines of equal level are drawn on the map at definite distances from each other, measured not from line to line on the map itself, but vertically from one horizontal plane to another; the lowest of these planes, or the *plane of reference*, being the level of the sea. To make the principle of this delineation, if necessary, still more clear, the shore-line itself, or the line where the land and the water meet, may be taken to represent, or may be regarded as being, the lowest, or *zero contour line.* Let then the water be supposed to rise to a level twenty feet higher, as it might actually do in some places, in the daily flow of the tide; and a new shore-line will thus be formed, running into the valleys and jutting out at the points of the hills. This would be the second contour line of the topographical map. Let another and another rise of equal amount take place successively, and a series of contour lines will be formed, which, when delineated on the map, will be near together where the natural slopes are steep, and far apart where they are gentle. Such are the lines which the survey determines. Drawn on the map, they show to an instructed eye the form of the ground more precisely than the most perfect model could do; and by far more satisfactorily than an inspection of the ground itself, which no eye or mind could grasp in one perfect whole; or, if it could, could estimate in regard to the relative level of points remote from each other horizontally, with any sort of precision. For that the ground itself, if it could

possibly be seen all at once by the engineer, might be to him all that the map is, in making up his judgments, it would need to have traced on it those very contour lines which are actually traced on the map.

The great practical use of such maps is, that a line of road can be at once, by their help, planned out in such a manner as to have the easiest grades and the gentlest curves, all obtained at the least cost that the work admits. Often, too, they surprise and delight the engineer, by indicating a line of gentle slopes and easy execution between important points already connected by a road running over high hills and descending into deep valleys. These revelations are made by the survey incidentally, no doubt, but necessarily, just as the mapping of its systematic soundings (a process precisely analogous) has already revealed important channels, before unknown, though leading into some of our most frequented harbors.

The advantages of such maps, however, can be but imperfectly realized by the Coast Survey, as its topography is restricted to a narrow belt along the coast; but the advantage which might accrue to the commonwealth, from complete topographical surveys of the interior, on the model of the Coast Survey, cannot be over-estimated. Of similar maps of Great Britain, a distinguished engineer, Mr. Vignoles, recently testified in the following emphatic words: "I can lay out on the map the general line of a railroad to within twenty or thirty yards, and say to my assistants, 'That is the general line: fill in the details;' — whereas, if I had not the advantage of that map, I must go to much trouble and expense in doing that which I here find already arranged to my hand." The same engineer speaks of the value of similar maps in the Grand Duchy of Baden; and adds, "In Switzerland I found some portions of the contours so accurate, that the line actually levelled, after I had sketched it out, differed scarcely at all from the section produced by plotting from the contours." These maps will even supply an approximate estimate of the cost of a proposed road; and by their portraiture of the country which it is to lay

open, will enable us to approach very nearly to its probable revenue and profits.

BEARING ON THE ADVANCEMENT OF SCIENCE IN GENERAL.—The influence of the Coast Survey in promoting scientific tastes, and in stimulating scientific activity in our country, has been unquestionably great. This influence has manifested itself not only in the zeal and interest which it has awakened in the study of abstract theory, but in the amount and refinement of skill which it has introduced in the application of theory to practice; not only in the spirit which it has diffused with regard to the observation of nature, but in the happy ingenuity which it has developed and set at work for the perfection of methods of observation. It has been itself, in the first place, a school of scientific practice, in which many of the younger officers of our navy have been thoroughly trained in all the details of hydrographic surveying, and in which officers of the military arm have been familiarized with the delicate operations of geodesy. Many civilians, also, have received similar scientific training in the same school, whose proficiency and enthusiasm in their profession have first directly profited the Government in connection with this great public work, and have afterwards, in their retirement from the service, carried into other spheres of action a species of attainment of the rarest, and in our country at the present day, of the most practically valuable kind.

The investigations undertaken by the Coast Survey, in special branches of physical inquiry, have likewise added greatly to the interest of the meetings of the Association, to which this committee owes its appointment; having been constantly prolific of communications possessing the combined merits of novelty, variety, and value. They have thus aided materially in keeping alive, and in stimulating to higher efforts, the spirit of scientific research among the members of our own body, and, through them, operating upon the tone of public sentiment in all those widely separated parts of the country from which they annually gather. Moreover, these investigations

have not merely thus served as a stimulus to intellectual activity, but they have presented models, worthy of all admiration, for the imitation of investigators; and have thus not only kindled among us the zeal to be serviceable to science, but have done very much to show us how we may be most so.

The scientific journals of our country are furthermore greatly indebted to the contributions not only of the chief of the survey, whose labors in this way have been abundant, but also of those associated with him in his work, and who, in their special departments, have brought to light new facts, or originated new inventions, or developed new theoretic views, which have added to the variety of our periodical scientific literature, and have assisted in advancing our scientific reputation abroad.

If any one particular branch of American science is more indebted to the operations of the Coast Survey for the life which has been instilled into it through their instrumentality than any other, it is probably astronomy. The astronomers of the country have been directly called on by the survey for their active coöperation in settling, in an authoritative manner, questions of latitude and longitude. They have responded to the call not only with cheerfulness, but with zeal; and they have found themselves, in consequence, unconsciously incited or allured into the performance of other labors which had not been demanded, and into the pursuit of new investigations, which had not before suggested themselves. It is certainly true that within the last fifteen or twenty years American astronomy has made great advances; but if this fact is recognized or admitted, it is impossible not to recognize at the same time the important agency of the Coast Survey in promoting its advancement.

It may finally be remarked, that the survey has recently even lent valuable aid to history by causing the preparation of a historical account of the progress of discovery on the western coast, as connected with its hydrography, and by collecting the maps, old and new, which illustrate this history. A work which, in so many ways, and with so abundant success, has

contributed to promote intellectual progress at home, and to secure for our country an honorable reputation abroad, cannot certainly fail to command the approving appreciation of every enlightened and patriotic citizen.

OPINIONS OF SCIENTIFIC AUTHORITIES ON THE COAST SURVEY.

When, some years ago, a proposition was introduced into the Senate of the United States to transfer the Coast Survey to the Navy Department, the principal scientific bodies throughout the country presented remonstrances against such a course. The American Academy of Arts and Sciences, the American Philosophical Society, and the Franklin Institute, of Pennsylvania, made elaborate reports, highly approving of the methods and commending the progress of the survey. The Faculty of St. John's College, of Maryland, and the Professors of the University of Virginia, addressed memorials to Congress in favor of the work as organized.

The American Academy of Arts and Sciences says:

"In conclusion, it is the deliberate opinion of the Committee that the present Superintendent of the United States Coast Survey has, by his able and judicious, his energetic and economical, administration of this great national work, raised it to the highest order of successful activity and deserved popularity; and that he has thereby fulfilled the high expectations which were raised at his appointment."

The American Philosophical Society states:

"That the survey of the coast is a work which, from its importance to our citizens, recommends itself in the strongest manner to the protection of the government. That the benefits of a scientific and practical character which have already been derived and are constantly resulting from it, are such as to repay abundantly the labor and expense which have been and may be hereafter devoted to it. That it has heretofore been conducted accurately efficiently, and economically, and that there is every reason to believe that it will best thrive by being left with its present organization. And that, as well from the magnitude of the undertaking as from the skill and energy with which it has been conducted, it will prove honorable to those who first conceived it, and to those who have been engaged in its prosecution."

This Society urges the publication of the observations upon which the maps and charts produced by the survey have been founded; and extends this recommendation to the methods used for the computation and discussion of the observations: a subject to which the attention of the government had been repeatedly drawn by the superintendent; and which it is, in the interests of science, sincerely to be hoped, will, at no distant day, be favorably acted on.

The Franklin Institute conclude their report with the following:

"First. That the work is national in its character, and ought to be continued by the Federal Government upon a scale commensurate with its practical importance, and with the extent and variety of the objects which are benefited by its results.

"Second. That the manner in which it is at present organized is economical, efficient, and scientific — giving its results with sufficient rapidity, at moderate cost, and upon scientific and practical principles.

"Third. That its labors ought to be (as they now are) divided among the civil, military, and naval talent of the country, in order to secure to each of these departments that knowledge of its processes and determinations which is equally required by them all.

"Fourth. That the benefits which the nation has already obtained from it have fully justified its cost, and that those benefits ought now to be continued to the whole of the States of the Union, upon as broad and liberal a scale as the government has hitherto applied."

The American Association for the Advancement of Science, at their meeting held in Cambridge in 1849, appointed a committee to consider the subject of the Coast Survey, whose report, entirely favorable to the administration of the work, was unanimously adopted by the scientific men then and there present from the different parts of the United States. Their opinions are expressed in the following resolution:

"*Resolved*, As a sense of the American Association for the Advancement of Science, that the manner in which the Coast Survey of the United States has been conducted by Professor A. D. Bache, and those associated with him, is highly creditable to American science, that it promises great benefit to the commerce and navigation of the country, and it is eminently entitled to the continued favor of Congress."

This Association have since testified their undiminished confidence in the superintendent of this work by electing him their president, the highest position which can be occupied by a scientific man in our country.

When the rumor reached Europe that the present organization of the Coast Survey was assailed, the venerable Humboldt, and his compeer of the French Academy of Sciences, Arago, wrote to their friend and correspondent, Professor Schumacher, of Altona, expressing flattering opinions of the manner in which the survey was conducted, of the attainments of the superintendent, and of the honor which our country had obtained abroad from the successful prosecution of this great work.

No name stands before that of ARAGO in the science of the world, and he speaks of the probable loss of Professor Bache's services as "a misfortune which he desires to unite with all that Europe has most distinguished in science, to prevent."

Baron HUMBOLDT, addressing Professor Schumacher, says:

"You know, better than I do, in how high an estimation the director of the work for the survey of the coast stands, not only among us, but among all the most illustrious men who in France and England are interested in the study of geography and nautical astronomy. To the most solid knowledge of astronomy and mathematics, Mr. Bache unites in a very eminent degree that activity of mind and extent of views which render a work of practical utility profitable to the science of the physics of the globe. In a region of the globe where the direction of oceanic currents, the differences of temperature produced by these currents and by the upheaval of the bottom and the direction of the magnetic curves, offer so important phenomena to the navigators, such a great work could not be placed in better hands than those of Dr. Bache. The government of the United States has acquired a new right to our gratitude by protecting nobly that which has arrested the attention of the hydrographers and astronomers of Europe. I should be glad to think that in a country where I am honored with so much good feeling, my feeble testimony might contribute to enliven the interest which is due to the excellent labors of Mr. Bache."

Professor SCHUMACHER, the learned astronomer of Altona, transmitting these letters, says:

"You will see, by the enclosed letters of our common friends, M. Arago and the Baron von Humboldt, how anxious they are to know if the great work you have undertaken will remain in the hands to which the whole scientific world would have entrusted it. Let me hope that the fears they entertain are only founded upon vague rumors. Your country must be proud to call you its own, and will repay you in gratitude what you do for her scientific glory."

The Baron von Humboldt very recently expressed to a member of this committee, in a personal interview, the highest appreciation of the Coast Survey of our country; and the warmest admiration of the manner in which it had been conducted, and of the peculiar qualifications of its chief.

Capt. (now Admiral) W. H. SMYTH, R. N., President of the Geographical Society of London in 1850, in speaking, in his address of that year, of the United States Coast Survey, says:

"The Coast Survey of the United States is a truly national undertaking, and has been most creditably conducted through all its various departments of science. I have studied the question closely, and do not hesitate to pronounce the conviction, that though the Americans were last in the field, they have, *per saltum*, leaped into the very front of the rank.

"Were I asked to give instances, I would say, look to their beautiful maps and charts; see their practice of establishing longitudes by electricity, and the probable extension of its wonderful chronographic applications; mark their novel method of taking and recording transits by a galvanic circuit; and consider the excellence and refinement of their astronomical observations for geodetic purposes, as proved by their being able to detect the alteration of gravity, caused by a difference in the density of the earth's crust."

The President of the same Society, in his address in 1852, while noticing with admiration the very efficient manner in which the survey has been conducted, and the rapidity with which the work has been carried on along the western shores of the continent, classes it as "one of the most *perfect exemplifications of applied science, of modern times.*"

And, finally, we may call attention to the emphatic tribute to the value of the Coast Survey, from Sir RODERICK MURCHISON, President of the Royal Geographical Society of London, on presenting the Victoria Gold Medal, adjudicated for the

year 1858 to Professor Bache, in consideration of "his successful labors in carrying out the great Coast Survey of the United States of America."

"It would be impossible," he says, "to do justice to an extensive work of this sort on an occasion like the present; but as the previous reports of this celebrated Coast Survey, from 1844 to 1855 inclusive, are in our library, those of our associates, and of the public generally, who wish to form an estimate of their value, can do so at their leisure, and they will see how vastly our medallist has pushed on this great work. They will assuredly then rise from the examination with the thorough conviction that, whether we regard the science, skill, and zeal of the operators, the perfection of their instruments, the able manner in which the Superintendent has enlisted all modern improvements into his service, the care taken to have the observations accurately registered, his modest and unpretending demeanor, or the noble liberality of the Government, tempered with prudent economy, all unprejudiced persons must agree that the Trigonometrical Survey of the United States of America stands without a superior."

## CONCLUSION.

With these voluntary and emphatic testimonies to the character of a work as magnificent in its scientific aspects as it is valuable in those which are purely utilitarian—testimonies, moreover, emanating from sources which rank, in point of authority among the highest known to the scientific world—the committee might be justified in closing a report already protracted beyond their expectation. After the extended review, however, which they have taken of the purposes in which this great undertaking originated, of the history of its growth, and the expansion of the processes involved in its execution, and of the brilliant results which have already crowned its diversified labors, it will probably be expected of them that they should condense the final expression of their opinions into a form sufficiently concise to be comprehended at a single view. As the succinct recapitulation, therefore, of the conclusions at which they have arrived, the committee, with entire unanimity, concur in stating the following propositions:

1. The American Coast Survey, in its inception, was a work imperatively demanded by a due regard to the industrial interests of the country, dependent, as they are, greatly upon the prosperity of commerce for their free development.

2. The indecision which marked the early policy of the government in regard to this survey, and the consequent delay of its efficient operations, and postponement of its beneficial results, were of manifest disadvantage to the material welfare of our people, and cannot but be still subjects of serious regret.

3. The economical value of such surveys is attested by the universal voice of all commercial men, and by the concurrent practice of all commercial nations, no less than by the melancholy records of marine disaster annually occurring upon every unexplored coast.

4. Their scientific value is witnessed, in the instance of the American survey, by the spontaneous tributes of approval frequently and freely bestowed upon it — no less in regard to the ability, energy, and skill displayed in its management, than to the magnitude, variety, and oftentimes curious interest of the results it has wrought out — by individuals and organized bodies of men, whose high position as scientific authorities renders their opinions upon subjects of this nature entirely conclusive.

5. This work has conferred many valuable benefits upon science, indirectly and incidentally, in the invention or perfection of instruments, in the improvement of methods of observation or of computation, in the development which it has given to special subjects of interesting inquiry, and in the stimulus which it has furnished to the scientific talent of the country, especially in the field of astronomical observation and investigation.

6. A careful study of the progress made from year to year, especially since the enlargement of the scale of operations under the present superintendent, affords ample evidence that the work has been expeditiously prosecuted, and that the amount accomplished up to the present date is materially greater than

has ever been accomplished in any other country in the same length of time, and with the same means.

7. Compared with similar surveys executed or in progress of execution by foreign governments, the American survey has been conducted with remarkable economy.

8. Compared with such foreign surveys, the quality of the work done in this will bear the test of any standard that has ever been anywhere set up, and is such as to reflect honor on the scientific character of our country in the eyes of the world.

9. Every consideration of economy, of humanity, and of regard for the reputation of the country, demands that the work should be prosecuted with undiminished activity, until every portion of our coast shall have been as thoroughly explored and mapped as those have been already in which its operations commenced.

10. Conclusive reasons, involving other weighty public interests no less than this, but connected also with the project of verifying in the happiest manner the geodesy of our extended and circuitous coast, conspire to render the triangulation of the great Appalachian chain of mountains a most desirable undertaking, and encourage the hope that our government will very early direct that most important work to be executed.

11. The publication in full of all the observations upon which the published results of the Coast Survey are founded, together with the methods employed in the reduction and discussion of the observations, would be a contribution to science, and especially to the science of geodesy, of inappreciable value, besides being necessary to secure the records against loss; and the committee earnestly hope that the government may not fail to provide the means for the adequate and rapid prosecution of this work.

12. The existing organization of the survey, judged in the light of the experience acquired by our own and by foreign governments in the management of such works, is, in the deliberate opinion of the committee, preferable to any other that has ever been suggested.

These propositions have not been hastily sketched, and are not lightly thrown out; but they are announced as the result of mature reflection and careful consideration. With their announcement, the duty of the committee, under the resolution appointing them, is discharged. The committee cannot, however, forget that they have another duty, unprescribed by any resolution, to fulfil; which is, to express, on behalf of the Association which has charged them with their present responsibility, and of the world of science, which they may claim for the moment to represent, their deep sense of the obligation which they feel to be due to the enlightened statesmen who, whether in the executive branch of the government, or in the legislative halls of Congress, have sustained the work to the present hour by their liberal recommendations or their able advocacy, and have labored to conciliate to it the popular favor by their intelligent and manly expositions of its objects and its value.

Among the distinguished men who hold in their hands the destinies of the country, are still to be found statesmen no less enlightened and no less liberal. To such, therefore, with whatever branch of the government they may be connected, the committee, in conclusion, most cordially commend the important work which they have been reviewing; and in the name of the associated science of the country, they solicit for it the continuance of the Executive favor and Legislative support which it has hitherto enjoyed.

F. A. P. BARNARD, Chairman.

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| JOSEPH G. TOTTEN, | W. H. C. BARTLETT, |
| BENJAMIN PEIRCE, | WOLCOTT GIBBS, |
| JOHN TORREY, | STEPHEN ALEXANDER, |
| JOSEPH HENRY, | LEWIS R. GIBBES, |
| JOHN F. FRAZER, | JOSEPH WINLOCK, |
| WILLIAM CHAUVENET, | JAMES PHILLIPS, |
| JOHN LECONTE, | WILLIAM FERREL, |
| WILLIAM M. GILLESPIE, | EDWARD HITCHCOCK, |
| F. H. SMITH, | JAMES D. DANA. |

# HISTORY OF THIS REPORT.

At the meeting of the "American Association for the Advancement of Science," held in Montreal, in August, 1857, a resolution was adopted, upon the recommendation of the Section on Physics, constituting the gentlemen immediately hereinafter named a committee to inquire and report on the Coast Survey of the United States:

1. JUDGE J. K. KANE, President Amer. Phil. Society, Penn.
2. GEN'L JOSEPH G. TOTTEN, Chief Engineer U. S. A.
3. PROF. BENJAMIN PEIRCE, Harvard College, Mass.
4. PROF. JOHN TORREY, U. S. Assay Office, N. Y.
5. PROF. JOSEPH HENRY, Sec'y Smithsonian Institution, D. C.
6. PROF. J. F. FRAZER, University of Pennsylvania, Penn.
7. PROF. WM. CHAUVENET, U. S. Naval Academy, Md.
8. PRES'T F. A. P. BARNARD, University of Mississippi, Miss.
9. PROF. JOHN LECONTE, College of South Carolina, S. C.
10. PROF. WM. M. GILLESPIE, Union College, N. Y.
11. PROF. F. H. SMITH, University of Virginia, Va.
12. PROF. W. H. C. BARTLETT, U. S. Military Academy, N. Y.
13. PROF. WOLCOTT GIBBS, Free Academy, N. Y.
14. PROF. STEPHEN ALEXANDER, College of New Jersey, N. J.
15. PROF. LEWIS R. GIBBES, Charleston College, S. C.
16. PROF. JOSEPH WINLOCK, Sup't Am. Naut. Alm., Ky.
17. PROF. JAMES PHILLIPS, University of North Carolina, N. C.
18. PROF. WM. FERREL, Nashville, Tenn.
19. PROF. EDWARD HITCHCOCK, Amherst College, Mass.
20. PROF. JAMES D. DANA, Yale College, Conn.

Upon receiving notice of this action by the Association, the chairman addressed a circular to his associates of the committee, in the following form:

SIR: — I beg leave to transmit to you the copy of a Resolution adopted by the AMERICAN ASSOCIATION at its last Meeting, and to invite your advice and guidance in the preparation of an appropriate Report to that body.

I have supposed that the several topics, which such a Report should embrace, may fitly arrange themselves under the following heads:

1. The character and primary objects of a Geodetic and Hydrographic Survey.

2. The action of other Governments in regard to such Surveys of their Territory and Coast.

3. The former organization of the Coast Survey of the United States — its progress and results up to the year 1844.

4. The Modifications our Coast Survey has undergone since — its expansion — the novel and improved methods and appliances of which the present Superintendent has availed himself, and the collateral researches which these have favored.

5. Its results up to the present time, general and particular — Their bearing: *First*, On navigation — facilitating the determination by the navigator of his position, by reference to soundings, tides, currents, and landmarks — indicating perils and positions of refuge — discovering and defining channels, and noting the changes they have undergone — and fixing the proper sites for light-houses and beacons: *Second*, On the protection of the coast — suggesting the localities for forts, docks, magazines, etc.: *Third*, On the geodetic surveys which may be proposed by the several States — facilitating them by furnishing a chain of base lines, and determining the absolute position of points: *Fourth*, On great works of communication — in the discussion of traces and gradients.

6. The judgments which have been expressed by scientific men in other countries, and our own, as to the value of the work and the efficiency of those who have it in charge.

While soliciting from you, however, such contributions as your leisure may permit, under any of these heads, I would not be understood as desiring to circumscribe the range of your communications, nor indeed as defining the subjects which should enter into the Report. It will be my object, unless otherwise instructed by the Committee, to submit to the Association, and through it to the community, a popular view of the objects and course of the Survey, and of the influence it is exerting, and may continue to exert, on different interests of the country; but in regard to this, as to all other points, I shall be happy to avail myself of your suggestions. It will be for you to determine how far they shall be incorporated in the general report, or whether they shall be recognized as proceeding from yourself.

I may be excused for adding, in conclusion, that I shall esteem it a great personal favor, if you will allow me to be assured of your coöperation at an early day.

I am, Sir, with very great respect,

Your obedient servant,

J. K. KANE.

To this circular the members of the committee replied very generally, some of them at large; and the chairman had already made some progress in the analysis of their contributions, and in arranging these, and the other materials gathered for his report, into an available shape, when his labors were arrested by his lamented death — an event at once calamitous to his country and to science, which, to the great grief of his associates, took place on the twenty-first of February, 1858.

At the meeting of the Association, held in Baltimore, in April, 1858, the papers of Judge Kane, relating to this subject, were exhibited to the members of the committee then present. As chairman in his place, President F. A. P. Barnard, of the University of Mississippi, was appointed, and it was agreed that the publication of the report ought to be made as soon as possible after its completion.

The new chairman not being in attendance at Baltimore, there occurred an unavoidable interval of some duration between the adjournment of the Association and the transfer of the papers to him. Since obtaining possession of them, however, he has been as expeditious in turning them to use as circumstances would allow, following in general the arrangement of topics laid down in the letter of Judge Kane, cited above; and now, in concluding his labor, he feels constrained to express his unfeigned regret that it had not been permitted to the abler hands to which the task was originally assigned, to execute the work which they had so judiciously planned.

To those of his associates on the committee, whose contributions relating to subjects embraced in the report communicated to the former chairman have passed into his possession, he begs leave here to tender his thanks for the valuable aid which they have afforded him.

UNIVERSITY OF MISSISSIPPI, OXFORD, Nov. 2, 1858.

# PROCEEDINGS

OF THE

# SPRINGFIELD MEETING, 1859.

## COMMUNICATIONS.

## A. MATHEMATICS AND PHYSICS.

### I. MATHEMATICS.

### 1. A New Method of Investigating Plane Curves, with its Application to Evolutes and Caustics. By William Watson, of Cambridge.

Last year, at Baltimore, in connection with the solution of a special problem, I presented to the consideration of the Association, some new systems of coördinates, which seemed to be of particular value in the investigation of certain classes of plane curves.

Since that time, having occasion to integrate certain differential equations, my mind was again turned towards the subject of curvature, and I was induced to examine the resources of some of the systems which I had proposed, in the hope of throwing some light upon my investigations.

I found that with some of the systems, curves could be accurately plotted, could be rectified, their quadrature found, their evolutes determined, and, in short, thoroughly discussed; while the differential equations which expressed the relation of the coördinates of a curve in

these, to those of the ordinary systems, were not susceptible of integration by any process known to the Calculus.

To those who object to the multiplication of methods of research in curvature as unnecessary, I shall reply by a quotation from the greatest living geometer of France.

"On ne saurait avoir trop de moyens différens de decrire une même courbe, parce que chacun exprime une propriété caractéristique de la courbe, d'où derivent naturellement plusieurs autres propriétés qui n'apparaissent par aussi aisément dans les autres modes de description." *

If to any point of a given plane curve [Fig. 1] we draw a radius vector, a tangent, and a radius of curvature; and also from the origin three lines respectively perpendicular to these, we shall have the following quantities geometrically represented: $r$ the radius vector; $\varphi$ the polar angle; $\varepsilon$ and $\tau$ the angles made by the tangent with the radius vector and the prime radius respectively; $\varrho$ the radius of curvature, and $\nu$ its angle with the prime radius; $p$ and $q$ the perpendiculars from the origin to the tangent and to the radius of curvature.

Since if one of the above quantities is fixed in any given curve, all the others assume definite values, each one will generally be a function of every other, and the curve may be expressed and defined by an equation between any two members of the series, thus giving rise to what have been called "New Systems of Coördinates."

Before taking up the special subject of this paper, it will be necessary to premise a few truths with reference to a system, which, although recognized in our treatises upon the calculus, has been used, rather as a form under which curves could be expressed, than as an instrument by which they could be fully discussed. I mean the system in which the general equation of a curve is $r = f(p)$; and I will briefly state what is known in reference to it.

1. The sign of $D_p r$ determines whether the curve is concave or convex toward the pole, and therefore there will be a point of contrary flexure for every change of its sign.

2. If $p$ has several values corresponding to one of $r$, for that value of the radius vector the curve has a multiple point.

3. If for any finite value of $p$, $r$ becomes infinite, the curve has an asymptote.

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* Aperçu Historique des Méthodes en Géométrie. — *Par M. Chasles.*

4. To find the equation of the evolute. Let the radius vector of the corresponding point of the evolute be denoted by $r_e$. Let

$$r = f(p) \tag{1}$$

be the equation of the curve, then from the geometry of the figure

$$r_e = \varrho^2 - 2p\varrho + r^2. \tag{2}$$

Also
$$p_e = r^2 - p^2, \tag{3}$$

$$\varrho = r D_p r. \tag{4}$$

From these four equations we obtain $r_e = F(p_e)$, the equation of the evolute.

5. To trace the curve we must know the angles $\varepsilon$ and $\tau$.

$$\varepsilon = \sin^{-1}\frac{p}{r}.$$

Since

$$q = \sqrt{r^2 - p^2} = D_\tau p,^*$$

$$\therefore \tau = \int_p \frac{1}{q} = \int_p \frac{1}{\sqrt{r^2 - p^2}}.$$

No constant need be added in this integration, as the direction of the prime radius may be so changed that the constant, if there were any, would disappear: we have now $\varphi = \tau - \varepsilon = \psi(p)$ from which, with $r = f(p)$, the curve may be constructed by points.

6. The formula for rectification is $s = \int_r \frac{r}{q}$.

7. The formula for quadrature is $A = \int_r \frac{pr}{q}$.

In the system which it is the design of this paper to develop, any

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* This relation was first distinctly stated and geometrically proved by Mr. Ferrers in the "Quarterly Journal of Mathematics," but may be shown true by analysis, thus,

$$\rho = \frac{r\,dr}{dp} \quad (\alpha) \qquad \rho = \frac{ds}{d\tau} \quad (\beta).$$

In which $ds$ represent an elements of a curve,

$$\therefore \frac{dp}{dr} = D_\tau p = \frac{r\,dr}{ds} = r\cos\varepsilon = q.$$

curve is expressed by an equation between $q$ and $\nu$; i. e. the general equation of any curve is $q = f(\nu)$.

For example, the equation of a cardioid $r = 2m \sin^2 \frac{1}{2}\varphi$, becomes, when transformed to this system, $q = m \sin \frac{2}{3}\left(\nu - \frac{\pi}{2}\right) \sin \frac{1}{3}\left(\nu - \frac{\pi}{2}\right)$, or $q = m \sin \frac{2}{3}\tau \sin \frac{1}{3}\tau$.*

The polar equation of the parabola referred to its focus and axis

$$r = \frac{2m}{1 + \cos \varphi} \text{ becomes } q = m \sec \nu \tan \nu.$$

The polar equation of the logarithmic spiral $\varphi = \log r$ or $r = e^{\phi}$ becomes when transformed $q = e^{(\nu - \frac{1}{2}\pi)} \cos \frac{\pi}{4}$.

1. The relation $\nu = \tau + \frac{\pi}{2}$ gives $p = \int_\tau q = \int_\nu q$.

From which we have

$$\varepsilon = \tan^{-1}\left(\frac{\int_\nu q}{q}\right) \text{ or } \cot \varepsilon = D_\nu \log \int_\nu q.$$

2. The radius vector $r = \sqrt{(\int_\nu q)^2 + q^2}$.

3. The radius of curvature $\varrho = p + q_e = \int_\nu q + D_\nu q$.

This relation furnishes a ready method of transforming the equation of a curve given in this system to "Peirce's Circular Coördinates."

Take, as an example of this, the equation of the parabola

$$q = m \sec \nu \tan \nu,$$
$$\int_\nu q = m \sec \nu,$$
$$D_\nu q = m \sec \nu (1 + 2 \tan^2 \nu),$$
$$\therefore \int = 2m \sec^3 \nu.$$

4. To find a point of contrary flexure. At such a point $\varrho = \infty$, and $p$ is finite; $\therefore D_\nu q = \infty$, while $\int_\nu q$ is finite.

5. For an asymptote $q = \infty$ and $\int_\nu q$ is finite.

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* The angle made by $q$ and the prime radius is $\tau$, which is sometimes more convenient than $\nu$ to use.

6. To find the equation of the evolute,

$$q=f(\nu). \tag{1}$$

From the geometry of the figure,

$$D_\nu q=q_e. \tag{2}$$

And

$$\nu_e=\nu+\frac{\pi}{2}. \tag{3}$$

To find the $n^{th}$ evolute $q_n=D_\nu^n q$, and $\nu^n=\nu+\frac{n\pi}{2}$.

To find the $n^{th}$ involute ${}_nq=\int_\nu^n q$, and ${}_n\nu=\nu-\frac{n\pi}{2}$.

Take as an example, to find the equation of the evolute, and that of the involute of the parabola,

$$q=m\sec\nu\tan\nu,$$

$$q_e=-m\operatorname{cosec}\nu_e(1+2\cot^2\nu_e),$$

$${}_1q=m\operatorname{cosec}{}_1\nu.$$

Thus it will be observed that evolutes and involutes are determined in this system as readily as in "Peirce's Circular Coördinates."

7. The formula for rectification, since,

$$\varrho\,d\nu=ds, \text{ is } s=\int_\nu^2 q+q.$$

8. The formula for quadrature is

$$A=\int p\,ds=\int p\,\varrho\,d\nu=\int_\nu\left\{(\int_\nu q)^2+D_\nu q\int_\nu q\right\}.$$

9. To plot the curve, use $\tau$ instead of $\nu$;

$$\left(\tau=\nu-\frac{\pi}{2}\right).$$

Lay off values of $q$ corresponding to different values of $\tau$, and at the extremity of each erect a perpendicular; also lay off at the origin the corresponding values of $s$ from $q$ toward the prime radius, and the intersection of each of the last series of lines, with the corresponding perpendicular will be a point of the curve.

## APPLICATION TO CAUSTICS BY REFLECTION.

### *Parallel Rays.* [Fig. 2.]

Assume the prime radius parallel to the incident rays. Let

$$q=f(\nu) \tag{1}$$

be the general equation of the reflecting curve.

From the geometry of the figure $\tau_c=2\tau$,

$$\therefore \nu_c=2\nu-90, \tag{2}$$

$$\therefore d\nu_c=2d\nu$$

$$p_c=r\sin(\pi-2\tau+\varphi)=r\sin(\tau+\epsilon)=r(\sin\tau\cos\epsilon+\cos\tau\sin\epsilon)$$

$$=q\sin\tau+p\cos\tau=q\cos\nu-\sin\nu\int_\nu q,$$

$$\therefore q_c=\tfrac{1}{2}D_\nu(q\cos\nu-\sin\nu\int_\nu q)=\tfrac{1}{2}\cos\nu D_\nu q-q\sin\nu-\tfrac{1}{2}\cos\nu\int_\nu q.$$

### *Rays not Parallel.* [Fig. 3.]

Let the origin be at the luminous point; then

$$\nu_\epsilon=\nu+\epsilon,$$

$$p_c=r\sin 2\epsilon=2r\sin\epsilon\cos\epsilon=\frac{2pq}{r}=\frac{2q\int_\nu q}{\left[(\int_\nu q)^2+q^2\right]^{\frac{1}{2}}},$$

$$q_c=D_{\nu_c}p_c.$$

Another system of coördinates which may be useful, is that in which the general equation of a curve is $p=f(q)$.

The investigation of this system would follow very simply from what has preceded, and the geometric construction would present no difficulty.

Usually the nature of the problem is sufficient to determine what system of coördinates is best adapted to the investigation.

All these systems, and indeed every possible algebraic form by which curves may be expressed, must be regarded as generalizations of that method, by which more than two hundred years ago Descartes changed the whole character of mathematical science.

Fig. 1.

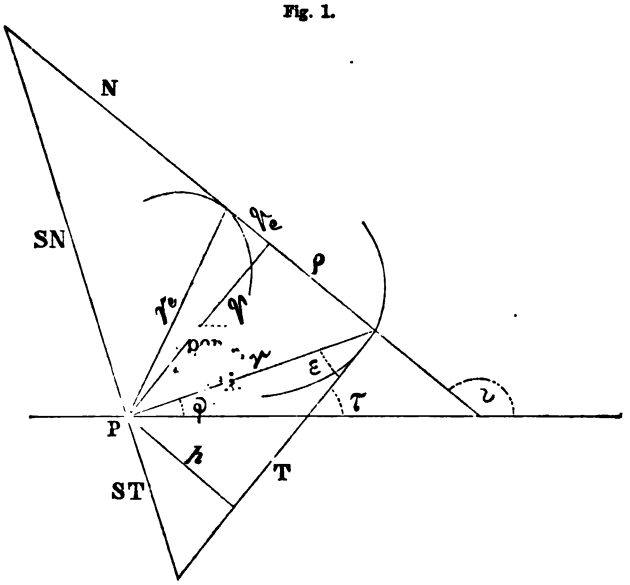



Fig. 2.

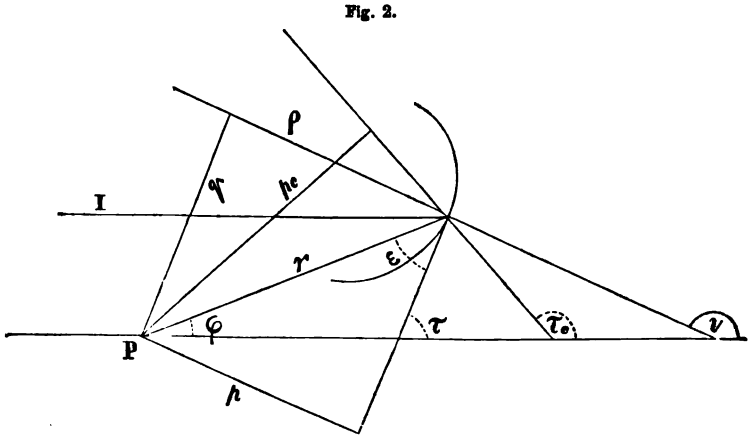
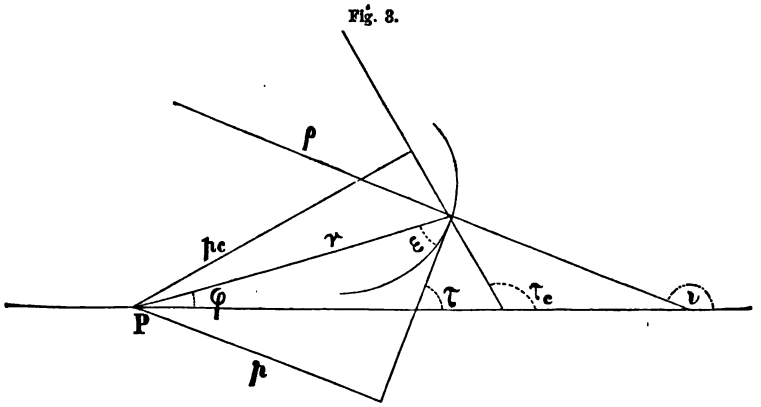



Fig. 3.



2. On certain Spirals. By Rev. Thomas Hill, of Waltham.

The great genius of Descartes and of Newton has so impressed upon us the idea of classifying curves by the degrees of their equation, or by putting them with trigonometric or logarithmic curves, that we hardly think other modes natural. Yet the algebraic parabola, the trigonometric cycloid, and the logarithmic catenary have a close geometric relation; since in each the normal measured from the curve to a given straight line bears a constant ratio to the radius of curvature.

I have discovered another group of spirals which have a still simpler property in common, and which nevertheless embrace among them the logarithmic spiral, a certain involute of an epicycle, and a certain second involute of a circle. This group of spirals answers the problem; What curve is that in which the radius of curvature is directly proportioned to the radius vector? — that is in which

$$\varrho = A\,r.$$

In transforming this curve a constant of integration, $B$, is introduced. When $A > 1$, and $B = 0$ the curve is a log. spiral, but when $B > < 0$ the log. spiraloid loops near the origin, or encloses the origin in a loop, and runs out in both directions.

When $A < 1$ and $B > < 0$ the curve is an involute of an epicycloid. The numerical ratio $\frac{A}{B}$ determines the ratio of the generating circles, and the length of the thread.

When $A = 1$, $\varrho = r$ is the second involute of the circle whose radius is $B$; the constant for the second involution being $\frac{1}{2}\,B$.

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## II. ASTRONOMY.

1. On the Secular Variations of the Eccentricities and Perihelia of certain of the Asteroids. By Simon Newcomb, of Cambridge.

It is well known that Dr. Olbers proposed to account for the existence of several small planets, where a single large one had naturally

been expected, by supposing that they were fragments of a large one, which had been shattered through the agency of some unknown cause. Latterly astronomers have not seemed disposed to look with much favor on this hypothesis; one of the principal arguments brought to bear against it being founded on the fact that there is no appearance of a common point of intersection, or common node to the orbits in their present state; that the perihelion distance of some is greater than the aphelion distance of others; and that the present rate of change of the orbits does not indicate that they were any nearer a common point of intersection formerly than at present. These facts however can scarcely be considered as militating against Olbers' hypothesis at all. The perihelia and nodes of most of the asteroids perform a complete revolution around the heavens in from 20,000 to 40,000 years; and the eccentricities pass through at least one maximum and one minimum in the same length of time. If, therefore, their orbits ever did intersect, they would not necessarily give any indication of such intersection after the lapse of 30,000 years. If then there is a space of three millions of years in which to look for the explosion of the planet (and I do not suppose that this period will be considered unreasonably long by astronomers or geologists), the chances would be a hundred to one against our finding it in the last 30,000 years, and consequently against our seeing any indication of it in the present positions of the orbits.

The mode of investigating the problem which seems most likely to lead to a conclusive result, is to make the actual computation of the nearly rigorous expressions for the eccentricities and perihelia of certain of the asteroids for all time. If we then substitute these expressions in the equations of condition which the elements must satisfy in order that the orbits may intersect, we can find whether at any time a common point of intersection was possible. In selecting the asteroids for which to make this computation, the following conditions were, as far as possible, complied with:

1. That neither their eccentricities nor inclinations should be large. This condition is necessary, because the third powers of those quantities have to be neglected in the calculations, and if they were not small, this neglect might render the results seriously defective.

2. That the present values of the elements should be tolerably well determined.

3. That they should include as wide a range as possible of mean distances.

The asteroids which seem best, on the whole, to fulfil these conditions, are Vesta, Metis, Hygea, and Parthenope.

What is commonly known as the secular variation of the orbit of a planet, is the apparently uniform progressive change which both theory and observation show to affect its form. As far as observation alone has shown, this change may continue for an indefinite period of time, and if so, it would finally lead to the destruction of our system by causing the planets to come into collision with each other or with the sun. Theoretical investigation, however, shows that this change is really a motion of oscillation, or rather of several superimposed oscillations, the periods of which, for the larger planets, vary from 50,000 to 500,000 years; and therefore, as I have before remarked, the present rate of change of the asteroid orbits throws but little light upon the question of their former intersection, because in the lapse of ages the direction of the change will be reversed. I have, therefore, computed the oscillations of the eccentricities and perihelions of the before-mentioned asteroids, with the following result:

Put $t$ for the number of Julian years after 1800, $t$ being negative before that epoch.

$e$ the eccentricity, $\pi$ the longitude of perihelion counted from the equinox of 1800.

$$(1) = 97^\circ\ 50' + 2''.90 \times t; \quad (2) = 25^\circ\ 52' + 5''.80 \times t,$$
$$(3) = 126^\circ\ 37' + 22''.82 \times t; \quad (4) = -45^\circ + 17''.86\, t,$$
$$A_1 = 230^\circ\ 56' + 38.47\, t; \quad A_2 = 85^\circ\ 40' + 39''.28\, t,$$
$$A_3 = 215^\circ\ 36' + 89''.20\, t; \quad A_4 = 247^\circ\ 12' + 42''.16\, t.$$

*We then have for Vesta,*

$$e \sin \pi = .1051 \sin A_1 + .0018 \sin (1) + .0254 \sin (2) - .0188 \sin (3) + .0002 \sin (4),$$
$$e \cos \pi = .1051 \cos A_1 + .0018 \cos (1) + .0254 \cos (2) - \&c.:$$

*For Metis,*

$$e \sin \pi = .1182 \sin A_2 + .0018 \sin (1) + .0256 \sin (2) - .0184 \sin (3) + .0002 \sin (4),$$
$$e \cos \pi = .1182 \cos A_2 + .0018 \cos (1) + .0256 \cos (2) - \&c.:$$

*For Hygea.*

$$e \sin \pi = .1310 \sin A_3 + .0022 \sin (1) + .0315 \sin (2) - .0139 \sin (3),$$
$$e \cos \pi = .1310 \cos A_3 + .0022 \cos (1) + .0315 \cos (2) - .0139 \cos (3):$$

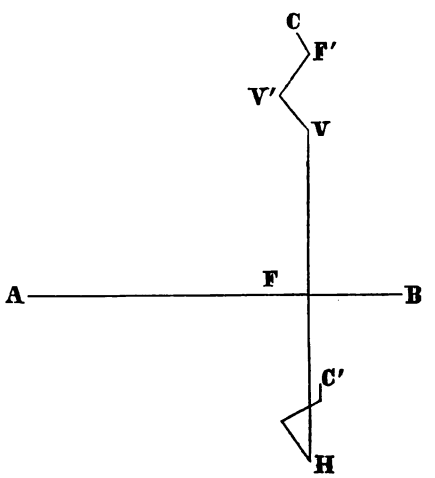
*For Parthenope,*

$$e \sin \pi = .0779 \sin A_4 + .0018 \sin (1) + .0262 \sin (2) - .0174 \sin (3) + .0002 \sin (4),$$

$$e \cos \pi = .0779 \cos A_4 + .0018 \cos (1) + .0262 \cos (2) - \&c.$$

Let us now see whether the orbits of Vesta and Hygea could ever have intersected while these equations hold true. By adding together the several coefficients for each asteroid, without regard to their algebraic signs, we obtain the limit which the several eccentricities can never exceed. For Vesta, this limit is .1513, and for Hygea, .1786. Whenever the eccentricities attain these limits, the aphelion distance of Vesta is 2.7194; and the perihelion distance of Hygea, 2.5868, 13 millions of miles less. So although the perihelion distance of Hygea is at present greater than the aphelion distance of Vesta, such was not always the case. This, however, is not sufficient to insure an intersection of the orbits. In order that this result may follow, it is also requisite that the perihelion of one planet should be somewhere near the aphelion of the other, and whenever this condition is fulfilled, it is impossible that the eccentricities of *both* planets should much exceed their mean value. This results from the fact that the coefficients of the common angles (1), (2), &c., have the same sign for the two planets. Perhaps the most convenient way of showing it is by geometrical construction of the above equations.

Let $F$ be the focus of the ellipse in which the sun is situated, $AB$ the line from which the longitudes are reckoned. Then $e \sin \pi$ and $e \cos \pi$ will represent the rectangular coördinates of the centre of the ellipse taken with the negative sign, the mean distance of the planet being the unit of length. If, therefore, we lay off from $F$ a line $FV = .1051$, and making an angle equal to $A_1$ with $FA$; from $V$ lay off $VV' = .0018$, making an angle $= (1)$ with $FA$, and so continue with



the remaining terms, the point $C$ finally arrived at will represent the position of the centre of the ellipse, and $CF$ will be its eccentricity. In order that the perihelion of the orbit of Hygea should be near the aphelion of that of Vesta, its centre must be nearly on the opposite side of the sun, the angle $A_3$ must therefore differ from $A_1$ by nearly 180°, and if we construct the terms corresponding to Hygea in the same manner that we did those corresponding to Vesta, the quantity .1310, which is the controlling term, must be laid off somewhere in the direction of $FH$. But since the remaining angles are the same as for Vesta, the quantities .0315 and .0179 must be laid off nearly toward the focus, and thus the eccentricity of Hygea, instead of being at its maximum, will be at its minimum, and therefore most unfavorable for intersection. Supposing the line $FH$ to revolve around $F$, it is evident that it will not be till the perihelions come within 90° of each other, that the eccentricity of Hygea can attain its mean value, and in order that it may attain its maximum, the perihelion must coincide with that of Vesta. The effect of these circumstances is to prevent the orbits of Vesta and Hygea from ever intersecting, as we might conclude from this geometrical construction, but in order to leave no doubt of the subject, the above expressions were substituted in the general algebraic equations, which must be satisfied in order to render the intersection possible, and it was found that the distance of Vesta would have to be increased by about 0.130, or 12 millions of miles, to render possible an intersection of its orbit with that of Hygea.

NOTE. — The methods and details of the preceding investigations are given in the Astronomical Journal, No. 129.

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## 2. DESCRIPTION OF A SELF-RECORDING INSTRUMENT FOR TAKING THE SUN'S ALTITUDE WITHOUT AN ARTIFICIAL OR NATURAL HORIZON. By JOHN OAKES, of New York.

It is well known that we have had no instrument that would closely approximate to the sun's altitude at sea when the natural horizon is not visible. The ordinary artificial horizon cannot be used on account of the motion. Many experiments have been tried with instruments designed to supply the want, but heretofore with little success. The great

difficulty was found to be, that the weight suspended so as in some manner to produce a horizontal line, or a zenith-point, to which to refer the sun, would necessarily partake somewhat of the motion of the vessel, and the altitude might be taken at either extremity of the vibration without the observer's knowledge or power to make any certain allowance for it.

In the instrument about to be described, it is believed this difficulty has been overcome, by its being *self-recording* and measuring the vibrations.

If a ray of light from the sun should strike a horizontal surface at any angle, and could pass through that surface, the angles on the lower side would be the same as those on the upper; now if *where* this sunlight impinged it produced a permanent point or mark, and we had an instrument to measure how many degrees and minutes it was from this mark to the horizontal surface through which the ray of light entered, it would give us the sun's angle above the horizon.

*This is the principle of the instrument exhibited.* The mechanical arrangements to carry out the plan are, a hollow hemisphere from say six to twelve inches in diameter, with a plate that fits in it with its upper surface even with the equator of the hemisphere. Through the centre of this plate is a small hole, say $\frac{1}{32}$ of an inch diameter, the lower side around the hole is bevelled away, leaving the edge very thin so as to allow the taking of a low angle. The inner surface of the hemisphere is plated with silver, to receive the fumes of iodine. The hemisphere is nicely hung in gimbals within a movable box or stand, and has four adjustable weights at the bottom, for the purpose of adjusting the upper surface or equator parallel to the horizon, which is done on shore, by means of a spirit level.

For finding the angle of the mark made by the sun through the small hole on the inner surface of the hemisphere, an instrument is necessary. This may be a circular divided plate, with vernier, to be placed within the hemisphere vertical to its equator,—or a microscopic tube, mounted on a plate or ring somewhat like a Transit, with circle attached to the axis for the divisions, this to be placed where the horizontal plate with the hole in, came out.

The principal adjustment necessary is, that the centre of the measuring instrument shall coincide with the hole through which the sun's rays pass. Any error of eccentricity can be corrected by a reversal of the reading circle.

inch for ten seconds of arc, and one micrometer-revolution equal to 78″. 74. Taking the star-places as given by each of the catalogues at hand — the British Association, Greenwich Twelve-Year, and Radcliffe, chiefly the former — the fifty-five observations give one hundred and five separate results for latitude. Rejecting none of the observations made, and assigning to each of the results the same weight — that is, making no discrimination between catalogues, or between errors of observation and of star-places — the mean of the whole makes the latitude of the point in New Haven, where the observations were taken, 41° 18′ 42″. 43. Probable error of a single result, ± 0″. 98; of the mean, ± 0″. 10.*

Twelve comparisons of observations of the same stars on different evenings give for probable error of a single observation ± 0″. 42; which being so much less than that of a single result for latitude, shows that the observations themselves are much more reliable than the catalogue places of the stars.

Three hundred and fifty-three pairs of stars observed with the zenith telescopes of the Coast Survey, by experienced observers, give an average probable error for a single result of 0″. 77, as appears from a paper by Prof. Bache in the last volume of the Proceedings of this Association. According to the same paper, "a probable error in observing of from ± 0″. 25 to ± 0″. 80 may be expected for one observation, according to the size and quality of the instrument and the ability of the observer." The above probable error for one observation of ± 0″. 42, is well within these limits, and smaller than was to have been expected, considering the size of the instrument, as compared with those of the Coast Survey, the run of its level, and the fact that it was constructed by the observer himself, an unpractised amateur, in the intervals of other pursuits; the observations, moreover, being the first of the kind which he had ever made. If the results, therefore, are not as good as those of the Coast Survey, this is their apology. With instruments similar in size and workmanship, there is no reason why results with the transit instrument should not be as good as those with the zenith telescope. The former is proposed as a substitute for the latter only in cases where economy, convenience, or other circumstances may render it desirable.

* The corresponding result carefully deduced from 136 observations of 90 pairs (47 in addition to the above), is 41° 18′ 42″. 48, with a probable error of ± 0″. 07; probable error of a single result, ± 0″. 87; of observation alone for a single observation, deduced from 100 repetitions, ± 0″. 47.

been expected, by supposing that they were fragments of a large one, which had been shattered through the agency of some unknown cause. Latterly astronomers have not seemed disposed to look with much favor on this hypothesis; one of the principal arguments brought to bear against it being founded on the fact that there is no appearance of a common point of intersection, or common node to the orbits in their present state; that the perihelion distance of some is greater than the aphelion distance of others; and that the present rate of change of the orbits does not indicate that they were any nearer a common point of intersection formerly than at present. These facts however can scarcely be considered as militating against Olbers' hypothesis at all. The perihelia and nodes of most of the asteroids perform a complete revolution around the heavens in from 20,000 to 40,000 years; and the eccentricities pass through at least one maximum and one minimum in the same length of time. If, therefore, their orbits ever did intersect, they would not necessarily give any indication of such intersection after the lapse of 30,000 years. If then there is a space of three millions of years in which to look for the explosion of the planet (and I do not suppose that this period will be considered unreasonably long by astronomers or geologists), the chances would be a hundred to one against our finding it in the last 30,000 years, and consequently against our seeing any indication of it in the present positions of the orbits.

The mode of investigating the problem which seems most likely to lead to a conclusive result, is to make the actual computation of the nearly rigorous expressions for the eccentricities and perihelia of certain of the asteroids for all time. If we then substitute these expressions in the equations of condition which the elements must satisfy in order that the orbits may intersect, we can find whether at any time a common point of intersection was possible. In selecting the asteroids for which to make this computation, the following conditions were, as far as possible, complied with:

1. That neither their eccentricities nor inclinations should be large. This condition is necessary, because the third powers of those quantities have to be neglected in the calculations, and if they were not small, this neglect might render the results seriously defective.

2. That the present values of the elements should be tolerably well determined.

3. That they should include as wide a range as possible of mean distances.

In November last, I was led to undertake a new line of investigation in relation to the physical constitution of comets. My attention being again especially drawn to the subject by the striking phenomena exhibited by Donati's comet, the idea occurred to me that the nebulous envelope of the head of a comet was probably in the same dynamical condition with the tail — that the cometic matter was, in the first instance, expelled from the nucleus on the side toward the sun, and then driven back to form the tail, by the repulsive force of the sun — that this process went on continuously; the nebulous matter rising perpetually, like a gushing fountain of light, and the luminous jets bending back, in graceful curves, before the repelling energy of the sun. According to this view, the nebulous envelope of the head, and the tail of a comet, are but different parts of the same flowing stream. The source of the luminous stream which we recognize as forming the tail of the comet, is thus carried back, in idea, from the nebulosity of the head to the bright nucleus lying within it. This general conception was subsequently developed into a systematic theory. The cometary matter was, in the first place, regarded as urged away from the nucleus by an incessant force of repulsion exerted by its mass; and this force, and the sun's repulsion, like all radiant actions, were supposed to vary agreeably to the law of the inverse squares. Other suppositions were afterwards made, — as that the expelling action was an instantaneous impulsive force; and again that the nebulous matter was first detached by a superficial force of projection, and subsequently acted on by the repulsion of the mass of the nucleus. The results of the investigation were communicated to the Connecticut Academy, at a meeting held on the 17th of November, 1858. A complete exposition of the process pursued, and results obtained, was afterward published in the January No. of the American Journal of Science, under the heading of "The Dynamical Condition of the Head of a Comet." In the article in question, the orbits of the receding particles, and the resulting form of the head of the comet, are approximately determined. It is shown that if we suppose the repulsive, or impulsive action, to be of the same intensity, from whatever point of the surface of the nucleus the cometic matter is ejected, the form of the head would be nearly that of a paraboloid, having its vertex directed toward the sun; but that if this force decreased in intensity in all directions from the point on the surface of the nucleus which is nearest the sun, the form would be

Insert pages following at this point and the article is then continued

# OFFICERS OF THE NEWPORT MEETING.

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Isaac Lea, Esq., *President.*
Dr. B. A. Gould, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. Joseph Leconte, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

*Standing Committee.*

Isaac Lea, Esq.,
Dr. B. A. Gould,
Prof. Joseph Leconte,
Dr. A. L. Elwyn,
Prof. Stephen Alexander,
Prof. Edward Hitchcock,
Prof. Joseph Lovering,
Prof. William Chauvenet.

*Local Committee.*

Prof. Wolcott Gibbs, *Chairman.*
Samuel Powell, Esq., *Secretary.*

Dr. David King,
Prof. Joseph Lovering,
Prof. Henry L. Eustis,
Prof. Alexis Caswell,
L. M. Rutherford, Esq.,
Prof. N. B. Hill.

## MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

| Meeting. | Date. | Place. | President. | Vice-President. | General Secretary. | Permanent Secretary. | Treasurer. |
|---|---|---|---|---|---|---|---|
| 1st, | Sept. 20, 1848, | Philadelphia, Pa., | W. C. Redfield, Esq., | | Prof. Walter R. Johnson, | | Prof. J. Wyman. |
| 2d, | Aug. 14, 1849, | Cambridge, Mass., | Prof. Joseph Henry, | | Prof. E. N. Horsford, | | Dr. A. L. Elwyn. |
| 3d, | March 12, 1850, | Charleston, S. C. | Prof. A. D. Bache,* | | Prof. L. R. Gibbes,* | | Dr. St. J. Ravenel.* |
| 4th, | Aug. 19, 1850, | New Haven, Ct., | Prof. A. D. Bache, | | E. C. Herrick, Esq., | | Dr. A. L. Elwyn. |
| 5th, | May 5, 1851, | Cincinnati, Ohio, | Prof. A. D. Bache, | | Prof. W. B. Rogers, | Prof. S. F. Baird, | Dr. A. L. Elwyn. |
| 6th, | Aug. 19, 1851, | Albany, N. Y., | Prof. L. Agassiz, | | Prof. W. B. Rogers, | Prof. S. F. Baird, | Dr. A. L. Elwyn. |
| 7th, | July 28, 1853, | Cleveland, Ohio, | Prof. Benj. Peirce, | | Prof. J. D. Dana, | Prof. S. F. Baird, | Dr. A. L. Elwyn. |
| 8th, | April 26, 1854, | Washington, D. C., | Prof. J. D. Dana, | | Prof. J. Lawrence Smith, | Prof. J. Lovering, | Dr. J. L. Le Conte.* |
| 9th, | Aug. 15, 1855, | Providence, R. I., | Prof. John Torry, | | Prof. Wolcott Gibbs, | Prof. J. Lovering, | Dr. A. L. Elwyn. |
| 10th, | Aug. 20, 1856, | Albany, N. Y., | Prof. James Hall, | | Dr. B. A. Gould, | Prof. J. Lovering, | Dr. A. L. Elwyn. |
| 11th, | Aug. 12, 1857, | Montreal, C. E., | Prof. J. W. Bailey, | Prof. Alexis Caswell, | Prof. John Leconte, | Prof. J. Lovering, | Dr. A. L. Elwyn. |
| 12th, | April 28, 1858, | Baltimore, Md., | Prof. A. Caswell,* | Prof. John E. Holbrook, | Prof. W. M. Gillespie, | Prof. J. Lovering, | Dr. A. L. Elwyn. |
| 13th, | Aug. 3, 1859, | Springfield, Mass., | Prof. S. Alexander, | Prof. Edw. Hitchcock, | Prof. William Chauvenet, | Prof. J. Lovering, | Dr. A. L. Elwyn. |

* In the absence of the regular officer.

# CONSTITUTION OF THE ASSOCIATION.*

## OBJECTS.

THE Association shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

## MEMBERS.

RULE 1. Members of scientific societies or learned bodies having in view any of the objects of this Association, and publishing transactions, shall be considered members on subscribing these rules.

RULE 2. Collegiate professors, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members on subscribing these rules.

RULE 3. Persons not embraced in the above provisions may

* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting.

become members of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

### OFFICERS.

RULE 4. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer, shall be elected at each meeting for the following one; — the three first-named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

### MEETINGS.

RULE 5. The Association shall meet, at such intervals as it may determine, for one week or longer — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

### STANDING COMMITTEE.

RULE 6. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large, who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be,—

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

### SECTIONS.

RULE 7. The Association shall be divided into two Sections, and as many sub-Sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

### SECTIONAL OFFICERS AND COMMITTEES.

RULE 8. On the first assembling of the Section, the members shall elect upon open nomination a permanent chairman and secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a chairman to preside over its meetings.

RULE 9. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committees may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

### REPORTS OF PROCEEDINGS.

RULE 10. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

### PAPERS AND COMMUNICATIONS.

RULE 11. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the chairman the titles of papers, of which abstracts have been received.

RULE 12. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Association.

RULE 13. Copies of all communications, made either to the General Association or to the Sections, must be furnished by

the authors; otherwise only the titles or abstracts shall appear in the published proceedings.

RULE 14. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 15. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 16. No exchanges shall be made between members without authority of the respective Sectional Committees.

### GENERAL AND EVENING MEETINGS.

RULE 17. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the chairman of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

### ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 18. The Association shall be called to order by the President of the preceding meeting, and this officer having resigned the chair to the President elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into Sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

### PERMANENT SECRETARY.

RULE 19. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee for correction.

### LOCAL COMMITTEE.

RULE 20. The Local Committee shall be appointed from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

### SUBSCRIPTIONS.

RULE 21. The amount of the subscription, at each meeting, of each member of the Association shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

No person shall be considered a member of the Association until the subscription for the meeting at which he is elected has been paid.

RULE 22. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

### ACCOUNTS.

RULE 23. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

### ALTERATIONS OF THE CONSTITUTION.

RULE 24. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

# RESOLUTIONS

OF A

# PERMANENT AND PROSPECTIVE CHARACTER,

ADOPTED AUGUST 19, 1857.

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1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also daily, during the meetings, provide the chairman of the two sectional committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to

receive copies of the papers read in their Sections, all sub-sections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A. M. daily during the meetings of the Association, to arrange the programmes of their respective Sections, including all sub-sections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A. M.

8. During the meetings of the Association the Standing Committee shall meet daily, Sundays excepted, at 9 A. M., and the Sections be called to order at 10 A. M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day, and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years, as they shall choose at the time of admission, — to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

ellipsoidal; and that it would be approximately spherical, if the expelling force decreased, from one point of the surface of the nucleus to another, according to the same law as the heating or luminous effect of the sun's rays falling upon the point. This was the form of the head of Donati's comet, and is that which has been ordinarily observed; but Halley's comet, as seen about two and a half months after its last perihelion passage, may be cited as an instance of a comet whose head had a parabolic form.

It is also shown that the other suppositions made with regard to the nature of the expelling action of the nucleus, give a similar form of head.

The attempt is also made to arrive at an approximate determination of the *masses* and *densities* of the nuclei of some of the brighter comets. The measurements that have been made of the distance of the vertex of the envelope from the nucleus, enable us to obtain the ratio of the repulsive force of the nucleus to that of the sun, and so to compare the masses and densities of the two bodies. The calculations were made for five different comets. The results obtained for the densities, lie between the numbers 4 and 14 (the density of water being 1). In some of the calculations, rough estimates of the distance of the comet from the sun, at the time of the observation, could only be obtained. The most reliable determinations, are those made for Donati's comet, which are between 4.4, and 5.9; or make the density of the nucleus of that comet from 5 to 6 times the density of water, or about the same as the mean density of the earth.

The calculations contain an element of uncertainty, arising from the apparent inequality of the expelling force of the nucleus, at different points of its surface, variously exposed to the sun. It is possible that this inequality may result from the fact of particles differently exposed to the sun's action having different degrees of susceptibility to repulsion. Upon this view it would have no effect upon the determinations of density. Upon any other view the results can only be regarded as rough approximations. They may still be relied upon, however, as affording sure indications that the telescopic nucleus of a bright comet is not entirely gaseous, or nebulous, but either liquid or solid, or composed of both liquid and solid matter, like the earth.

The article alluded to was in press before the second portion of Professor Bond's valuable memoir on Donati's Comet, in which the highly

interesting facts are given concerning the formation and expansion of a series of envelopes, came to hand. The recess of each of the envelopes, from the nucleus, at the rate of from six hundred to one thousand miles per day, observed by Prof. Bond, would seem, at first sight, to indicate that the envelope was a cloudy mass expelled bodily from the nucleus at that rate. But to this idea there appears to be an insuperable objection. Each envelope continued visible, without material diminution of brightness, for a number of days. Eventually its matter was dissipated by the sun's repulsive action. The brightness of the envelope was not much greater than that of the tail immediately proceeding from it, from which we may infer that at any one time it did not contain much more matter than was posited in an equal space within the tail, in its immediate vicinity. In the light of this fact how are we to explain the fact first mentioned, viz.: that the envelope retained nearly the same brightness for several days; if we suppose it to have been one connected mass, wholly detached from the nucleus, and receiving no fresh supplies from it, to replenish its stock of materials, rapidly passing away into the tail. In fact, the approximate equality, generally observed to subsist between the brightness of the nebulous envelope of a comet and that of the tail in its vicinity, taken in connection with the fact that the matter of the tail is perpetually flowing away from the head, most decidedly indicates that the tail is but the continuation of the stream flowing through the head, and issuing from the nucleus as its primary source.

The rise and gradual recess from the nucleus of a *succession of envelopes* may be explained, upon the dynamical theory, by supposing that the evolution of cometic matter first begins at the polar regions of the nucleus, and gradually extends toward the equatorial parts. By the polar regions is here meant the portion of the surface lying around the points 90° degrees distant from the plane of the orbit. In fact, the nucleus will apparently rotate about an axis perpendicular to the plane of the orbit, *so far as exposure to the sun is concerned.* Under these circumstances, if we view the comet under a large angle to the plane of the orbit, as was the case with Donati's comet, after the perihelion passage, the outline of the visible envelope would steadily enlarge, as the process of evolution was propagated further and further from the polar regions of the nucleus. This explanation supposes that *the envelopes are not spherical in form*, but that each approximates in its form

to a flat semicircular disc, of a certain thickness. That this was actually the approximate form of the envelopes of Donati's Comet may be inferred from the phenomenon of a very dark stripe repeatedly noticed by Prof. Bond, as extending a certain distance from the nucleus along the axis of the tail. This stripe was too broad and too long to have been the shadow of the actual solid nucleus; and it was also too narrow and dark to have been the hollow space in the middle of the tail, if we suppose the tail to have been round and hollow. I have reason to think that Prof. Bond concurs with me in the opinion that the axial stripe which he carefully observed and measured, affords an almost infallible indication that the envelopes from which the tail proceeded were much flattened in the direction of the line of sight; or approximately in that direction.*

In the idea here suggested, that the evolution of nebulous matter may be confined, for a time, to certain zones or districts of the surface of the nucleus, we have also a plausible explanation of the fact stated by Prof. Bond, that on the 6th of September, "when the earth was close to the plane of the comet's orbit, was first noticed the curvature of the tail;" upon which he remarks that "the observation cannot be reconciled with the commonly received opinion that the curvature of the tail lies in the plane of motion about the sun." In other words, the tail was, at the time of the observation, curved in a plane inclined to the plane of the orbit. This would have been the case, upon the present theory, if the tail had been made up of cometary matter ejected from parts of the nucleus lying entirely on one side of the plane of the orbit. To make this evident, let us suppose a jet to stream out from a point on the surface of the nucleus 90° distant from that plane; it is easy to see that the repulsion of the sun would bend it into a curve concave toward the plane of the orbit.

The results of the investigation of which I have now given a brief

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* This conception, in relation to the probable form of a cometary envelope, is implied in the explanation of the envelopes, given in the article published in the Journal of Science, "On the Dynamical Condition of the Head of a Comet," p. 102. The idea here presented, that the flattened form of envelope, supposed in the dynamical theory of the head, was infallibly indicated, in the case of Donati's comet, by the phenomenon of the dark axial stripe, was first expressly stated in a letter addressed by the author to Professor G. P. Bond, two or three months since.

exposition, derive an additional interest from the fact that they lend support to the idea of a *general force of cosmical repulsion*, exerted by all cosmical masses, and operating under certain circumstances upon matter in an exceedingly attenuated state. This force is probably really of the nature of an impulsive action; that is, is made up of impulses propagated by the ether of space, and taking effect against the surface of the inconceivably minute particles. It is easy to see that such propagated impulses, if they really exist, must be more effective in imparting accelerated motion to the detached particles of cometary matter in proportion as these particles are more minute. In this way we may perhaps explain the phenomenon of nearly straight "supplementary rays" presented by Donati's comet, and by other comets. The matter flowing off in these rays must have had a much higher velocity than that escaping in the principal tail, and this may have resulted simply from the more attenuated condition of this matter.

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## 5. Recent Observations, by various persons, on the Gegenschein, or Completed Arch of the Zodiacal Light. By Rev. George Jones, U. S. Navy.

My object in appearing before the Association, at this time, is to present the observations of several other persons on the gegenschein or completed arch of the zodiacal light, of which I have received accounts since our meeting in Montreal. The fact of the complete, unbroken arch of this light from the eastern to the western horizon, is not only a new one in such observations, but it also is of the highest consequence as regards conclusions; and its importance will, I trust, excuse me if, in exhibiting the whole subject, I repeat (cursorily) some things stated two years ago.

On the 17th and 18th March, 1803, Baron Humboldt, while observing the western zodiacal light, in the evening, off the western coast of America, in lat. about 6° N., noticed a light also in the eastern sky, as if a continuation of the former. It was also seen by the sailors on board his ship. Humboldt supposed it to be a reflection from the western zodiacal light then shining with great brilliancy, and called it *gegenschein*, a term which, for the sake of distinction where there is actu-

ally a difference, we may find it well to preserve. I shall use the word *gegenschein* (or *shining opposite*) in this paper; and by it I mean a light distinct in color and appearance from what in books has usually been called the zodiacal light. Let us take, for instance, an evening observation under circumstances favorable to the full development of the gegenschein; — the time we will say, an hour and a half after sunset. There will then be in the west, a strong, yellow light, nearly pyramidal in shape, the light strongest at the horizon, and diminishing in intensity as our eye ascends toward the vertex of the pyramid. There, however, the light does not end in a point; but it changes now into a belt of *white* light, with parallel borders, which belt passes over to the eastern horizon, its axis or central line nearly on the ecliptic; and thus, with the yellow pyramidal light, we have a complete arch from east to west, passing usually in or near our zenith. Towards dawn, these appearances are exactly reversed, the pyramidal, yellow light being now at the east, but the complete arch is still preserved. This *white belt* I shall, in this paper, call *the gegenschein:* the yellow, pyramidal light I shall call by the old name of zodiacal light, since, as just remarked, we may find it best to keep a distinction between the two; although, in my opinion, they are both from the same ring, the *zodiacal light* being from the portion of the ring towards the sun, where, consequently, the reflected sunlight is strong enough near the horizon, to show the extreme borders of the ring, and thus to give the light there a pyramidal shape. The gegenschein has always appeared to me as a white light, exactly resembling that of the milky way, and with parallel borders, except at its lower extremity, where it mixes with the glare of the horizon, and seems to spread a little. At midnight, the light is all gegenschein, except at the two extremities on the horizon, where, at that hour, the sun's clearly reflected light shows itself: an effect somewhat similar takes place, indeed, at all the deep hours of the night. Whether the gegenschein is *reflected* light, or whether it is *independent light from self-luminous matter*, is a subject open to discussion, and a most important one. For, if it is independent light, as I am strongly inclined to think it is, then we have an item gained in that most interesting question of the qualities of nebulous matter.

I return to the *historical* facts in the case.

In December 1853, and March 1854, the gegenschein was observed by Theodore J. C. A. Brorsen, observer at the observatory of Senften-

berg, in Bohemia, who published notices of it in "the department of astronomy, geography, and meteorology, formerly edited by Jahn, and now by Prof. Heis, in the annual of 1854, folios 38 and 156."* The same work contains subsequent notices of this subject by Brorsen.

I made observations myself on the gegenschein, on July 7th and 9th, and August 5th, of 1853, in the eastern seas, which are noticed in the 3d Vol. of the Japan Expedition; and I saw it also, subsequently, during that cruise. But my most satisfactory observations on this subject — observations which, in their interesting character, go far beyond all those in the Japan expedition, were made on the Andes, near the equator, whither I went in the summer of 1856, in order to observe further on the zodiacal light. I had no particular expectations respecting the gegenschein; and therefore was the more gratified as well as surprised when, at my second observation on those heights of 10,000 feet above the sea, I saw the complete arch of light across the sky, as if a belt of thin, white gauze were drawn over the heavens from east to west, and so distinct as to look like another milky way. I saw this, *at every observation, during seven months on the Andes;* and when moon and clouds did not interfere, *at every hour of the night;* so that, on these latter occasions, one night's set of observations gave me the complete circle of the heavens, except 20° on each side of the sun. The gegenschein, in all these cases, had an equal width throughout; was a white light like that of the milky way; and was strongest at its axial or central line, whence it dimmed off towards its edges or sides.† On three separate occasions, individuals, who had never observed the zodiacal light before, on going out with me to my observations, saw the complete arch at once: and, at my request, gave its boundaries among the stars, which boundaries very nearly corresponded to those I was myself in the habit of drawing on my astronomical charts.

After my return home, I sent blank charts of the skies to several friends, with requests that they would observe for this complete arch and make tracings of its boundaries: and my more especial object now is, to give the results, with such other subsequent facts as have come into my possession. The first of these which I shall lay before the

* His letter to me.

† An abstract of these observations on the Andes, has been published in the Amer. Journal of Science, No. 72, new series.

Association, is a letter I have received from Prof. Brorsen, dated Senftenberg, Bohemia, January 10, 1858. I will give only its most important details. He says:

"This zodiacal light and the phenomenon entitled by Baron Humboldt, the gegenschein (Kosmos, Vol. III. p. 589), I observe at this place on every clear evening, when not prevented by moonlight, with the exception of the months of June and July, when the twilight is too bright, and also the months of December and January, when, in both these months, the light falls on the milky way, and is extinguished by its splendor. I take the liberty of giving a brief record of some of the most remarkable appearances of these phenomena, which I have observed during the last year.

"1857, April 17. I never before saw the zodiacal light and the gegenschein so uniform and strong in their whole extension, shining up to 180° from the sun. The gegenschein had a pyramidal form between the $9^h$ and $10^h$ in the evening, with its base turned towards the S. E. horizon; in Leo it showed itself, only a very little dimmer however, on the S. border, and throughout and everywhere more sharply defined than on the northern border.

"April 18. The gegenschein very beautiful; the brightest spot in the same was very prominent.

"August 14, 4. The brighter place of the gegenschein $AR$ $21^h$ $43^m$. $\delta - 14° 0'$. Place of the sun's centre $AR$ $9^h$ $37^m$. $\delta + 14° 2'$.

"Aug. 22, 4. The brightest spot of the gegenschein $AR$ $22^h$ $10^m$ $\delta - 10° 5'$. Place of the sun's centre $AR$ $10^h$ $7^m$. $\delta + 11° 6'$.

* * * * * *

"1857, September 23, $10^h$. The gegenschein of the zod. light very beautiful and distinct, in a reddish glimmer between the upper end of the northern fish-band and the westernmost of the two fishes, $\pi$, $\nu$, $\varsigma$ of the fish and $i$ ceti; the brightest part of the gegenschein is quite near the vernal equinoctial point. Of the prolongation of the same along the ecliptic east and west towards the milky way, I could not observe any vestige at present.

"September 25. The gegenschein very beautiful and even more conspicuous than on the 23d; about the same position and appearance as on the 23d; a continuation in the ecliptic E. and W. not perceptible. The brightest point of the gegenschein is $AR$ $0^h$ $11^m$. $\delta + 1°$. The place of the sun's centre $AR$ $12^h$ $9^m$. $\delta - 1°$.

"October 13. The gegenschein of the zodiacal light between the bands of the fishes is a pale reddish, and its prolongation visible as far as the zodiacal light toward the S. W. along the ecliptic.

"October 15. The gegenschein between $\alpha$, $\nu$, $\mu$, $\varepsilon$ Piscium is very striking, of a reddish tinge; the prolongation westward in the ecliptic scarce perceptible.

"October 24. The gegenschein very beautiful in a somewhat reddish light below $\alpha$ and $\beta$ Arietis; the prolongation toward the horizon to Capricorn very distinct and of a pyramidal appearance."

In three respects, Prof. Brorsen's observations differ from my own. 1. He speaks of the gegenschein as having a pale reddish tinge: to my eye it always appeared as a pale white light, exactly like that of the milky way. It was as if a thin white gauze were drawn, arch-shaped, across the sky. 2. He speaks of a *brightest* spot, i. e. a spot brighter than the rest, almost directly opposite the sun. His publication (1854) in the "Astronomische Nachrichten," p. 998, adverted to such a fact, and led me, in my observations on the Andes, to give particular attention to this subject. At first I, night after night, ran my eye along the gegenschien without having previously taken pains to ascertain where, in the sky, the point opposite the sun might be: but I could see no brightest spot. Then again, I took pains, before going out, to find where, among the stars, was the sun's *opposition;* and I examined this place carefully for a spot brighter than the rest of the gegenschein; but was never able to discern any such. 3. Prof. Brorsen speaks of a spreading out of the gegenschein, pyramidically, at the horizon opposite the sun. I could never see this, except what was produced by the usual white glare near the horizon; with the exception also that in the deep hours of the night, when the sun was now acting at both horizons so as to make the *zodiacal light proper* overpower the gegenschein, there was then a small pyramid of yellowish light at each horizon. I have scarcely a doubt that the pyramidal form referred to by Prof. Brorsen was owing to these causes. All these three subjects are to be matter of further particular attention by observers.

Mr. Edward C. Herrick, of New Haven, Conn., has, for many years, given close attention to the zodiacal light; and I was anxious to engage him as an observer of the gegenschein, if it could be seen as far north as that. Under date of April 17, 1858, he has written me, "I am much obliged to you for your favor of the 8th inst., and,

on almost every favorable night since its reception I have looked out for the zodiacal light in the east [the gegenschein], about nine o'clock, P. M. I also notified Profs. Olmsted, Dana, and Newton, and other gentlemen, and requested them to observe likewise. Here, in the midst of the city, we find much embarrassment in observations on so faint a light, at this hour, from the illuminations of the gas-lamps in the streets. On two of the nights, moreover, we have had more or less of the aurora borealis.

"My general result is, that, in the most favorable circumstances, there seems to be a slight milkiness along the ecliptic in Cancer and Leo, but I cannot trace any certain connection between this and the strong western zodiacal light."

April 20th, same year, he wrote. . . . "Observations for the zodiacal light in the eastern sky, in the evening, were made by several of us here, independently, on several occasions about a month since. The general conclusion arrived at was this:— That a very faint illumination could be traced about the ecliptic, not quite to the horizon, and on the northern limb mingling with light from a region much further north. I was disappointed in its general faintness, and in my inability to trace the connection from the western zodiacal light. Part of the difficulty I attribute to the glare of the gas-lights, which we cannot escape without going a long distance."

April 30th, same year, he wrote: "I have to-day received from my friend and former work-fellow, Francis Bradley (now of Chicago, Ill.), a statement of his recent observations on the evening zodiacal light in the east, in which you will doubtless feel much interest. In his letter, dated 27th inst., he says: 'You will be pleased to hear that I have seen that light in the east, or something which resembles it both in position and shape. It is only less bright than in the west. I have seen it repeatedly; and on one evening took a brother out with me, who has had no experience in looking after such things (although he has been a sailor, and is really far-sighted), telling him only that there was a light somewhere in the east running up from the horizon. He soon detected it. I had no doubt before. But this was a confirmation of the fact. March 11th, I had a good view of it both west and east, but could not trace it overhead. Its altitude seemed to be about the same on either side. The milky way, beehive, and a few bright stars, interfered in the region overhead, and, at best, the sky looked only mot-

tled. It lay decidedly north of the ecliptic on both sides, east and west.'"

The atmosphere in Texas has a remarkable transparency, particularly after "northers;" and the German settlers there, as noticed in Olmsted's travels, say that the skies seem to be nearer to them in Texas than in any other part of the world. The southern portions of that State have also the ecliptic almost vertical to them in the evening, in March; and always the nearer we can bring the ecliptic to our zenith, the more satisfactory do our observations on the zodiacal light and gegenschein become. In latitudes further from the equator, we look more *slantingly or sidewise* through this nebulous matter, and it is therefore less decidedly marked on the sky. I was accordingly very desirous of having observations in Texas; and I have found a willing and efficient co-laborer in Col. C. G. Forshey, head of the Military Institute at Rutersville, lat. 29° 58′ N., lon. 96° 46′ W. I sent to him blank star-charts, with a request that he would make tracings on them of the boundaries of this light among the stars, and would forward some of these to me. In a letter from him under date of April 8th, 1858, he says: "For two months and more I have continually seen the complete girdle of the zodiacal light whenever the weather and moon permitted, and find now to my satisfaction that I have witnessed the same all winter, but was afraid to allege it, even in my own journal. But when I found my little children able to discern this secondary milky way in the heavens, I no longer doubted."*

March 2d, 1859, he wrote: "Your favor has been some days in hand, but for a month past we have had no suitable night for observation until this evening. I have just traced a chart for you, and have accompanied it by one of last year of the same date.

"The former of these exhibits very plainly the '*river of light*' across the entire heavens, with its axis a very little north of the ecliptic in the western sky, but quite far from it east of the galaxy. The chart last year

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* In this letter he says, also: "If you will pardon the abruptness of the thing, I will here inquire whether you have observed the tendency of the sporadic meteors to run down the zodiacal light? I have repeatedly made this note. Is it because my eyes are so constantly turned that way? That would not account for the direction they so often take." In answer to this query, I may mention, that I have often thought that I observed the same thing, but I am still doubtful of the fact.

was projected very soon after I had detected the light in its full extent, and does not give the axis very definitively. A great number of observations made since that time, have confirmed the location as shown in to night's observation — that the axis from Gemini to Scorpio, lies considerably north of the ecliptic, perhaps 3° or even more.

"You will perceive too, from the observation at $7^h$ $35^m$ P. M., the axis does not run exactly parallel to the ecliptic, and hence the meteor seems not symmetrically placed in reference to the sun. Many observations of mine give like results."

Col. Forshey has sent me three charts, with tracings on them, giving the boundaries of the zodiacal light proper, and of the gegenschein. The latter are parallel, and make that belt, or "river of light," about 11° in width. In my observations at Quito, the gegenschein presented itself with its strongest light along its central or axial part, thence diminishing in brightness on either side for some degrees, when there was sometimes a sudden and great dimming, after which the light gradually dimmed, until it entirely ceased: so that it looked as if there might have been *two* belts, one of a stronger light lying against or impressed on a dimmer exterior one, but both having the same axes. Col. Forshey's belt, as traced on his charts, corresponds very nearly or quite with my stronger or inner belt as seen at Quito, the other or fainter one probably not being perceptible to him in Texas. Indeed I could not always make out this sudden dimming at Quito, and it is not always noted on my charts. The average of my Quito observations showed that this belt of light crosses the ecliptic at an angle of 3° 20′, the nodes being in lon. 62° and 242°, the ascending node being in 62°, so that in our evening sky there in April, the portion of the belt visible to me was north of the ecliptic. Col. Forshey's observation that the axis of the gegenschein was then north of the ecliptic at a distance of about 3° 30′, but was not parallel to it, had a very close coincidence with my own, as have also the width of the belt and the parallelism of its boundary lines.

In the annotations on one of his charts, April 4, 1859, 9 P. M., he remarks: "The zodiacal light reached entirely across the sky, which is clear though not very bright. . . . Four cadets, who had never seen the zodiacal light, traced the column from the milky way to the foot of Virgo [120°, nearly all of gegenschein]. Its axis passes through or very near $\eta$ Leonis. The existence of this meteor now as an

*entire circle*, is beyond question with me, and it has doubtless always been so, but only lately observed with precision."

March 2d, 1859, his annotations say: "At 11, P. M., the light is undoubtedly visible through the whole eastern heavens up to the head of Leo, and in the west still visible up to the milky way, and dimly seen across the galaxy to Gemini."

These observations in Texas are very satisfactory. At Annapolis, Md., where I have resided since my return from Quito, I have repeatedly seen, in the spring, the complete arch of the zodiacal light and gegenschein, each with its peculiar color as on the Andes; but the boundaries were so indistinct that I have not made any effort at tracing them on my charts. Prof. Chauvenet, at the Naval Academy in that city, saw portions of the gegenschein on several occasions; and one night about 10 o'clock — the atmosphere remarkably clear — I was walking with the rector of the Episcopal church in that city, and with a lady well known through our country for her philanthropic efforts, conducted with force of intellect as well as excellence of heart; and on my happening to look toward the sky, I exclaimed at the distinctness and brightness of this gegenschein girdle. They both also saw it at once, and were able easily to trace it quite across the heavens from east to west.

## CONCLUSIONS.

1. *I think, then, that the existence of such a complete arch of light across the sky from east to west may be set down as a clearly and fully established fact.* To recapitulate the proof: a part, if not the whole, of the gegenschein connecting itself with the zodiacal light was seen by Humboldt; the whole arch has been seen by Prof. Brorsen in numerous instances, and careful observations on its various appearances have been made by him; I saw it on the Andes for seven months, every night, and at every hour of the night, when clouds or the moon did not interfere; and sometimes having repeated observations through the same night from the evening to the morning crepuscule, I saw thus, in one night, the COMPLETE CIRCLE of this light, with the exception of about 40° next the sun. In all these instances, and also at Payta, in Peru, at Panama, and off Cuba, I made tracings on my charts of the boundaries of the light. This complete arch was also seen, on the

Andes, at Payta, and at Panama, by three other different observers, who had never given attention to the subject before, but to whom the arch was so distinct that they all were able to trace its boundaries among the stars, and I was able, by their dictation, to mark these boundaries on my charts. The arch has also been seen by Mr. Bradley and his brother, at Chicago; Mr. Herrick and Prof. Chauvenet have clearly made out parts of the gegenschein; the whole arch was distinctly seen at Annapolis by two persons who had never observed on the subject before; and finally, Col. Forshey has repeatedly seen it in Texas, and has given me tracings of its boundaries. The cadets in his school and his children were also able to follow its markings on the sky. There is, thus, an amount of evidence that appears to place the subject beyond a doubt.

2. The boundaries of the gegenschein as I have seen it in all my observations on the Andes, and elsewhere, WERE PARALLEL. Thus they are also in the tracings dictated to me by the three persons just adverted to, on the Andes, at Payta, and at Panama (in the last case by Lieut. Wyman, U. S. N.); and so also are the tracings sent to me from Texas by Col. Forshey — at least, the latter differ at their extreme ends by only a single degree.

But if this gegenschein circle, or even semicircle, has this parallelism of boundaries, then there seems to be only one conclusion as to relative position to us, namely, that it must in all its parts, be *equidistant*, or, in other words, is a GEOCENTRIC RING. The rules of perspective will scarcely admit of any other conclusion than this.

3. It would seem that the *gegenschein* represents the denser portions of this ring, while the zodiacal light with its yellow tinge gives to us *the full extent* of its boundaries; its portions of greater tenuity at the edges being invisible to us except by the reflected light from the sun.

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## 6. On the Supposed Lunar Origin of Aerolites. By B. A. Gould, Jr., of Cambridge.

A THEORY frequently urged, if indeed not generally prevalent, considers aerolites as products of lunar volcanic eruption, thrown to the

earth by the original eruptive force, which propels them beyond the sphere of the moon's dominant attraction.

Now the assumption, that an aerolite has thus fallen directly to the earth, implies limits to the direction in which it must have left the moon, as also limits to the force with which it must have been projected in any given direction. And it follows that for any projectile force, assumed as constant, there are limits within which the mass of the body must be contained, in order that it may reach the earth. The examination of these limits is a very simple problem, and its results appear decisive in their bearing upon the theory upon which the assumption is founded.

The force of gravity at the moon's surface being about one sixth of that at the surface of the earth, any thing ejected from a lunar volcano would rise to a height far greater than that to which it would be thrown by a terrestrial volcano of equal power. The absence of any considerable atmosphere on the obverse side of the moon would increase the proportional height; and it is conceded that bodies sent from the moon, earthward, with a velocity of about one mile and a half a second, would pass out of the region where the lunar gravitation predominates over the terrestrial, and would become obedient to the earth's attraction. This requisite velocity is by no means an improbable one, being decidedly less than has been observed in masses ejected from terrestrial volcanoes, and only three times as great as that claimed for the new rifled ordnance; so that no objection on this score can reasonably be urged against the theory under consideration. Let us, however, examine the consequences to which the adoption of this theory would necessarily lead us.

1. Let us suppose a projectile expelled from the moon's western limb, in a direction just opposite that of the lunar motion, and consider the velocity required in order that the superior attraction of the moon may be overcome by the expelling force, and the body left equally free to describe a geocentric or a selenocentric orbit. The moon's mass being about $\frac{1}{80}$ that of the earth, we have for the selenocentric motion of the body, according to the fundamental formula of orbital motion,

$$v^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right) = \frac{1}{40}\left(\frac{1}{r} - \frac{1}{2a}\right).$$

The last term being constant for any orbit, we may put

$$v^2 = \frac{1}{40r} - C,$$

and if $h$ be the distance from the moon at which her attracting force is equalled by that of the earth, so that the orbital motion of the moon is all that remains to the ejected mass, we have

$$1 = \frac{1}{40h} - C,$$

and consequently,

$$v^2 = \frac{1}{40r} - \frac{1}{40h}.$$

Now at the moon's surface $r$ is, in round numbers, about $\frac{1}{240}$, using the moon's mean distance from the earth as unity, so that the velocity of projection will be

$$v^2 = 1 + 6 - \frac{1}{40h}$$

the mean velocity and distance of the moon in her orbit being the units. The distance $h$ cannot well be assumed as less than $\frac{1}{40}$, which corresponds to $v^2 = 6$; and all greater values of $h$ would imply greater values for $v^2$.

2. The same fundamental formula

$$v^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right)$$

answers the question as to the force of projection which would send a body from the moon to the earth's surface. To produce this result it is of course necessary that the body should describe a conic section, of which the perigee is within the globe of the earth; or, in other words, with a perigee-distance less than the earth's radius. This we may call in round numbers, four thousand miles. Putting the projected mass as unity, we have for the body when first beginning to fall earthward, and consequently at apogee,

$$r = a(1 + e)$$

so that

$$v^2 = \frac{2}{r} - \frac{1}{a} = \frac{1}{a}\cdot\frac{1-e}{1+e} = \frac{1}{a}\cdot\frac{\Pi}{A}$$

in which $\Pi$ and $A$ denote the distances at perigee and apogee respectively. That the body may strike the earth's surface vertically, the perigee-distance must be 0; that it may graze the earth at a tangent, this distance

must be four thousand miles; that an aerolite may fall from the moon to the earth at all, it is requisite that the perigee fall between these limits of distance.

For the maximum perigee, we have

$$\frac{\Pi}{A} = \frac{4000}{250000},$$

or roughly $\frac{1}{60}$, so that the square of the corresponding initial geocentric velocity would be

$$v^2 = \frac{1}{60} \cdot \frac{1}{a},$$

and since, if we neglect the eccentricity of the lunar orbit, the moon's velocity is given by the expression,

$$V^2 = \frac{1}{a'} = \frac{1}{2a},$$

we have with sufficient approximation

$$v^2 = \frac{1}{30} V^2$$

as the measure of the maximum initial geocentric velocity.

Now that the body projected from the western limb, in the plane of the orbit, may abandon the moon and obey the earth's attraction, it has been shown that its minimum velocity of original projection must be not less than $v = \sqrt{6}\, V$. These are the most favorable circumstances, and taking this value as correct, we find the limits of projectile velocity from the moon to be

$$v^2 > 6\ V^2$$

$$< 6\ V^2 + \tfrac{1}{30}\ V^2 = 6\ V^2 (1 + \tfrac{1}{180}).$$

It is thus manifest that, whatever the force of propulsion which would just enable a body to pass from any part of the moon directly to the earth, an increase of this force by about its $\frac{1}{180}$ part would render it impossible again. This change of $\frac{1}{180}$ in the force corresponds to a total change of $\frac{1}{360}$ in the velocity, or to a variation of $\pm \frac{1}{700}$ from the mean velocity of aerolites; which would, upon the assumed theory, bring the mass to perigee at a distance of 2,000 miles from the earth's centre of gravity. Any increase or diminution greater than this would preclude the possibility of the aerolite's striking the earth at all.

3. In the next place, it is not a difficult task to find limits for the region upon the moon's surface, within which the volcanoes must be situated, if the aerolites come to us from them. We are at once relieved from the necessity of considering any part of the moon's eastern hemisphere, since all masses ejected from that side must be affected with the moon's orbital velocity in addition to the component of the projectile impulse in the same direction, and thus hopelessly precluded from finding a perigee within the body of the earth. That an ejected mass should reach the earth from the centre of the moon's disc, would require an infinite velocity; and it is evident that the portion of the obverse lunar hemisphere within which the volcanoes sought must be situated is approximately in the form of a half-lune, with its vertex at the centre of the disc, and its greatest width at the western limb. What is this greatest width? Manifestly not very great; and if we consider volcanoes of any power assumed as constant and alike in all, the relative forces required for different volcanoes would be in the exact ratio of the secants of their selenocentric latitudes. Since the maximum force may only be diminished by its $\frac{1}{100}$ part, it will be seen that the limits are situated where

$$\sec \delta = 1 + \tfrac{1}{100},$$
$$\delta = \pm 6°,$$

so that if a volcano acting with unvarying power on equal masses at the most favorable part of the lunar surface exerts on them the maximum force requisite for sending them to the earth, no volcano exerting just the same power could ever send us its lava masses of equal magnitude, did its selenocentric latitude exceed 6°.

4. Finally, we find analogous limits for the mass ejected, prescribed by the same considerations. The square of the velocity imparted being proportional to the mass, or to the cube of the diameter, while the projectile impulse is proportional to the area, or to the square of the diameter, it follows that the force of propulsion imparted by any volcano of constant power is simply in the inverse ratio of the diameter of the body ejected. A globular mass of twice the diameter would require double the force in order to throw it to the same distance, and we have here again a limitation to the size of the aerolite, provided all other conditions are favorable. Its diameter may not be increased or diminished by $\frac{1}{700}$ part without a change of more than 4,000 miles in the

perigee distance; a volcano therefore which is situated in the plane of the orbit, and acting with a constant force can send a lava mass of given dimensions to the earth, would be unable to do this if its selenocentric latitude were changed by an amount where maximum is 6°, or if its longitude were enough changed to throw it out of a very small lune, or if either the force exerted or the diameter of the body were changed by their $\frac{1}{180}$ part, the other elements remaining constant.

Among so varying elements it is of course impracticable to assign definite limits to the force of the mass or the position of volcanoes which may send as their products, but this is by no means necessary for our argument. We are, however, warranted in inferring the following conditions as necessary:

For any force which can throw to the earth an aerolite of given mass, the whole extent of variation may not exceed its $\frac{1}{180}$ part; for any mass which, when impelled by a given eruptive force can reach the earth, the whole extent of variation may not exceed $\frac{1}{180}$ of its diameter; for any position on the moon, whence a given eruptive force will throw a given mass to the earth, the whole extent of variation is comprised within an area of $\frac{1}{40}$ or $\frac{1}{50}$ of the lunar surface.

From the foregoing considerations we are warranted in assuming that, for every body expelled from lunar volcanoes with a force adapted for throwing to the earth an aerolite of average dimensions, there are, in probability, at the very least 180 bodies expelled with forces not thus adapted; that, for every mass ejected with the average force of a lunar volcano and striking the earth, there are at least 180 masses of inadequate dimensions ejected; and that, for any given combination of volcanic force and projected mass, the region of the lunar surface whence the mass may reach the earth is exceeded in extent by the tract of lunar surface whence this would be impossible, in the ratio of about 50 to 1. Combining these several individual probabilities, it will readily be perceived that not more than

$$\frac{1}{50} \times \frac{1}{180} \times \frac{1}{180} = \frac{1}{1620000}$$

of all the ejected lava masses, or about 3 in 5,000,000 of each possible size, would probably ever reach the earth as aerolites. Nor is this an unsafe estimate. It is a very guarded one; and the fraction $\frac{1}{2000000}$ would be more likely to be correct.

Now, can we regard it as probable that the moon has parted with so

large an amount of matter as, nearly if not quite, two million times the combined mass of all the aerolites which have fallen to the earth? I think not. Even of those known to have fallen, there are more than 500, of various weights, one of them having a mass of 30,000 pounds. The tokens of such a loss of gravitative matter as this would imply, could not fail to be legibly inscribed in the unerring, and enduring records of our system. They would preclude the accordance, which is found to exist, between the present lunar theory and the ancient observations. They would be found inconsistent with the known values of precession and nutation. They might, indeed, almost be said to be incompatible with the present mass of the moon.

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## III. PHYSICS AND CHEMISTRY.

### 1. The Correlation of Physical, Chemical, and Vital Force, and the Conservation of Force in Vital Phenomena. By Prof. Joseph LeConte, of Columbia, South Carolina.

Matter constantly changes its *form* — but is itself indestructible except by the same power which called it into being. The same quantity of matter exists in the universe at all times. So also force changes its *form* constantly, but is itself indestructible, incapable of increase or diminution, and the same absolute amount of force exists in the universe at all times and forever. The mutual convertibility of the various forms of force is called "correlation of forces." The invariability of the absolute amount in the midst of constant change is called "conservation of force." This principle of correlation and conservation of force must be looked upon as one of the grandest generalizations in modern science, a principle startling at first, but when clearly understood and firmly grasped, almost axiomatic. It must be considered a necessary truth, and as such is a legitimate basis of deductive reasoning.

The correlation of *physical* forces is universally recognized as a principle in science, and not only so but has already been productive of

many beautiful and useful *results;* but the correlation of physical and *vital* forces while generally recognized as a probable fact has only been speculated on in a vague and as yet unfruitful manner. The science of life is scarcely yet ripe for the legitimate extension of this principle over its domain. The most elaborate attempt of this kind which I have seen, is contained in the very remarkable and suggestive paper of Dr. Carpenter, entitled "Mutual Relation of Physical and Vital Forces," and published in Phil. Trans. for the year 1850.

In the present paper I wish simply to present a few thoughts, which have originated in my own mind, in the course of reflection on this subject, in the hope that they may prove suggestive to others. They have at least the merit of being uninfluenced by the writings of others, — and therefore, perhaps, of presenting the subject in a somewhat new light. I sincerely wish I could present the matter in a more definite form, but it is certain that where a subject is not perfectly understood, the attempt to give our ideas more definiteness also makes them more questionable. We are obliged to be content with a certain vagueness, in the hope that by the use of right methods a clearness will come after. We must gratefully accept the twilight in the hope that it marks the approach of the full light of day.

There are four planes of material existence, which may be regarded as being raised one above the other. The *first* and lowest is the plane of elementary existence, the *second* the plane of chemical compounds, or mineral kingdom, *third*, the plane of vegetable existence, and *fourth*, of animal existence. Now, it is apparently impossible for any known force in nature to raise matter through all these grades at once. On the contrary there is a special force adapted for the elevation of matter from each plane to the plane above. It is the special function of chemical affinity to raise matter from plane No. 1 to No. 2. All the changes, too, which take place upon plane No. 2 by the mutual reactions of bodies situated on that plane, are under the guidance and control of this force. It is the special prerogative of the force of vegetation — of vegetable life, to lift matter from No. 2 to No. 3, i. e., from the condition of mineral matter to the higher condition of vegetable matter. All the changes which take place upon this plane, the laws of which constitute vegetable physiology, are under the guidance of this force. Finally, the force of animal life and that alone enjoys the privilege of lifting matter still higher into the 4th plane, i. e., the plane of animal exist-

ence. No force in nature can lift from No. 1 to No. 3, or from No. 2 to No. 4. Plants cannot feed entirely upon elementary matter, nor can animals feed upon mineral matter. The reason of this will be seen in the sequel. Thus it seems that after matter is raised from the elementary to the mineral condition, it requires an additional force of another and peculiar kind to raise it into the vegetable kingdom, and again another accession of force to raise it into the animal kingdom. These kingdoms are, therefore, truly represented as successive planes raised one above the other, thus:

*No.* 4. *Animal Kingdom.*
3. *Vegetable Kingdom.*
2. *Mineral Kingdom.*
1. *Elements.*

If, then, it be admitted that this is the relative position of these planes — that it requires a greater and greater expenditure of force to maintain matter upon each successive plane, then it follows *that any amount of matter returning to a lower plane by decomposition must set free or develop a force which may under favorable circumstances raise other matter from a lower to a higher condition.* Or, to express it by a mechanical illustration, a given amount of matter falling from one plane to any plane below, develops a force sufficient to raise an equal quantity of matter an equal height. Thus *decomposition* must in every case *develop force*, which force may take the form of heat as in combustion, or electricity as in electrolysis, or may expend itself in forming chemical compounds or *even in organizing matter.*

Again, in the same manner as matter may be arranged in several distinct and graduated kingdoms, so it seems to me the forces of nature may also be properly divided into distinct groups arranged in a similar manner one above the other. These are the *physical*, the *chemical*, and the *vital* forces. And as in the case of matter so also in the case of *force*, it is impossible to pass directly from the lowest to the highest group without passing through the intermediate group. The conversion of *physical* into *vital* force seems impossible without passing through the intermediate condition of *chemical force.*

These are the simple principles upon which are based all that follows — principles which may possibly seem fanciful to some unfamiliar with the principle of conservation of force, but the number of phenom-

ena which they consistently explain will I hope entitle them to serious thought.

1st. It is well known that chemical elements, in what is called the "nascent condition," i. e. at the moment of liberation from previous combination, exhibit a peculiar energy of chemical affinity not exhibited under other circumstances. It seems to me that this is readily explicable on the principle of conservation of force. At the moment of decomposition the chemical affinity which bound the elements together and which was before satisfied, is suddenly left unsatisfied. There is an attraction *set free* which was before *disguised* — a force *liberated* which was before *latent.* If conditions favorable are present this force may preserve the form of chemical affinity, and expend itself in forming other chemical compounds; or even, as we shall see hereafter, in organizing matter. But if favorable conditions are not present, then it may take some other form of force, e. g. heat or electricity, and *therefore no longer exist as chemical affinity.* The chemical affinity is said to be lost. To return to the mechanical illustration used above. Matter falling from plane No. 2 to plane No. 1 develops force sufficient to raise *other matter* from plane No. 1 to No. 2, but which in the absence of such matter may expend itself in heat or electricity or some other form of physical force.

2d. It is a fact, now well established, that the *seed* in germination forms carbonic acid, and in doing so loses weight. That is, the organized matter of the seed is *partially decomposed,* a portion of its carbon uniting with the oxygen of the air to form carbonic acid. Now it is this *decomposition* which develops the force by which germination is effected. A portion of the organic matter of the seed is *decomposed.* This decomposition sets free a force which suffices to organize the rest. The force necessary and therefore the amount of decomposition necessary in this case is small because the work to be accomplished is simply the change from one form of organic matter to another, or rather from *organic* to *organized* matter — to recur again to the former illustration, merely shifting a certain quantity of matter from one place to another upon the plane No. 3. "But how," it may be asked, "is this decomposition brought about?" This seems to be effected by the heat, and perhaps (according to Hunt) by the actinic rays, of the sun.* Heat and

* See Report by Robert Hunt on the Growth of Plants, Rep. Brit. Ass., 1846, p. 33, 1847, p. 30.

actinic rays have been spoken of by many writers, e. g. by Carpenter and by Robert Hunt, as the physical force which is changed into organizing force by means of the "substratum of an organized structure;" but the peculiarity of the view which I now present, is that this conversion does not take place *immediately*, but only *through the mediation of another force more nearly allied to the vital*, namely, *chemical force*. The food is laid up in the seed mostly in the form of starch. In the act of germination this starch is changed into sugar. Starch, as is well known, differs from sugar in two important respects, namely, it is *insoluble* and it is more *highly carbonized*.* Now, according to the ordinary view, the only object of the partial decomposition is to change the food from an insoluble to a soluble form — and this can be done only by elimination of a portion of the carbon in the form of carbonic acid. According to the view which I now present, *the food is always laid up in a more highly carbonized condition than is wanted, in order that force may be set free by elimination of superfluous carbon*. According to the ordinary view, if an insoluble food could be found, capable of conversion into the soluble form, without loss of carbon; then germination of the seed might take place without loss of weight, by the direct conversion of heat into vital force. According to my view, *decomposition*, and therefore *loss of weight, is absolutely necessary to develop the organizing force*, the loss of weight being in fact the exact measure of that force.

3d. As soon as the plant develops *green leaves*, a complete change takes place in its mode of development. It no longer loses weight, but increases in weight. It not only develops, but *grows*. The reason of this is, that the organizing force is no longer developed by decomposition of food laid up within its own tissues, but by the decomposition of food taken *ab externo*. Sunlight is universally admitted to be the physical force concerned in this decomposition. Further, it is generally supposed that there is a direct and immediate conversion of light into vital force in the green leaves of plants. But evidently this is impossible, since the *work done by the light is the separation of the two elements, carbon and oxygen*. Light is therefore converted into *motion*. It is therefore the chemical affinity thus set free which is the force im-

* Robert Hunt, Rep. Brit. Ass., 1847, p. 20–22. Carpenter, Comp. Phys., p. 288. Mülder, Chem. An. and Veg. Phys., pp. 208, 230.

mediately converted into vital force. The food of plants consists of carbonic acid, water, and ammonia ($CO_2$, HO and $NH_3$), or in some cases, according to M. Ville, of $CO_2$, HO and N.* Sunlight acting through the medium of the green leaves of plants has the remarkable power of decomposing $CO_2$. The *force thus set free from a latent condition*, or the chemical affinity of *carbon in a nascent condition*, is the force by means of which C, H, O and N are raised to the organic condition.† To return to my former illustration; matter (oxygen) falling from the second to the first plane, develops force sufficient to raise other matter from the second to the third plane. Thus it is evidently *impossible on the principle of conservation of force that plants should feed entirely upon elementary matter;* whereas according to the ordinary view of the direct conversion of light into organizing force, there is no reason why plants should not feed entirely on elements, except that one of them, carbon, is insoluble.

4th. There are many other phenomena of vegetable life which receive a ready explanation on this theory. I have said that sunlight has the power of decomposing carbonic acid only in the *green* leaves of plants. *Pale plants*, such as the Fungi among cryptogams and the Monotropa among phænogams, have no power to decompose $CO_2$. These plants, therefore, cannot feed upon chemical compounds — mineral matter. They *must feed upon organic matter*, which organic matter *in its partial decomposition furnishes the force necessary for organization.* If so, then this decomposition, as in the case of germination, must be attended with the elimination of $CO_2$. Both of these are known to be facts. Pale plants do feed upon organic matter and do evolve $CO_2$. The necessary connection of these facts with one another and with the principle of conservation of force, is now for the first time, as far as I know, brought out. The phenomena of nutrition in

* See review of the controversy between Boussingault and Ville on this subject, Bib. Univ. Arch. des Scien., vol. xxx. p. 305. Also Phil. Mag., 4th ser. vol. xiii. p. 497. Ann. des Scien., 4th series, vol. ii. p. 357. Am. Jour. Science, vol. xix. p. 409. Bib. Univ. Arch. des Scien., vol. xxviii. p. 335. Ann. des Scien., 4th series, vol. vii. p. 5.

† Ammonia is also probably decomposed in the tissues of the leaves of plants (Carpenter, correlation of physical and vital forces, Phil. Trans., 1850, p. 732. See also, Morren, Bib. Univ. Arch. des Scien., new period, vol. v. p. 84). This would of course produce additional organizing force.

these plants is similar to that of seeds in germination, except that the latter contains the organic matter already laid up within its own tissues, while the former derives it from decaying vegetable or animal matter taken *ab externo* into its tissues. In this case too, as in germination, heat is apparently the physical force which effects the decomposition of the organic food, and which is therefore converted indirectly through chemical into vital force. Light is actually unfavorable to this process, for light tends to decompose, not to form $CO_2$. In both cases, therefore, the conditions favorable for nutrition are first, abundance of soluble organic matter; second, absence of light and presence of heat. This is then apparently the true reason why germinating plants and pale plants avoid the light. The plants grow by the *oxydation* of carbon and formation of $CO_2$. Light *decomposes* $CO_2$, and must therefore be antagonistic to its formation, and consequently to the growth of these plants. Whether or not this property of light is entirely limited by the condition of its acting through an organic tissue, is a question yet undetermined. Heat we know is favorable to the oxydation of carbon (combustion, fermentation, putrefaction, etc.) under all circumstances. Has light an opposite property also under all circumstances, or is this opposite property of light limited to the condition of its acting through the medium of an organism? I hope the experiments already commenced and still in progress, by my brother, Prof. John LeConte, and published in the last Proceedings and in the American Journal of Science and Arts, vol. xxiv. p. 317, will eventually furnish the means of solving this very important problem. I do not wish to anticipate the final results of these experiments, but it seems to me that the negative results thus far obtained, rather support the view that the action of light is not thus limited. In all experiments on this subject, the light and heat of the sun have been combined. Now, heat we know is favorable to combustion. The fact, then, that combined light and heat produced no effect, would seem to indicate that light counteracted the effect of the heat of the sun.

5th. *Etiolated plants*, or plants artificially blanched by exclusion of light, exhibit the same phenomena and for the same reason. These plants cannot receive their organizing force through the decomposition of $CO_2$ by sunlight: therefore they are obliged to obtain it from decomposition of organic matter. Hence these plants require organic food, hence also they evolve $CO_2$ instead of oxygen. In this case also,

decomposition of organic matter, with a separation of a portion of the carbon in the form of $CO_2$, furnishes the organizing force. In the absence of any external organic matter in the form of humus or manure, etiolated plants, like germinating seeds, will feed for awhile upon organic matter previously accumulated in their tissues in the form of starch, and actually *lose weight* of solid matter.*

6th. In a most interesting and suggestive article in the Bibliothèque Universelle (Archive des Sciences,†) on the subject of humus, M. Risler shows in the most conclusive manner that organic matter in a soluble condition (soluble humus) is taken up by *almost all plants*. This fact had been previously proved experimentally by Th. de Saussure, but having been denied by Liebig, it has been very generally neglected by vegetable physiologists. The doctrine of Liebig and of physiologists generally, is that, except in case of pale plants, organic matter is decomposed into $CO_2$, HO and $NH_3$, i. e., must fall into the mineral kingdom before it can be absorbed and assimilated by plants, and therefore that organic manures only supply the same substances, and in exactly the same form, which are already supplied, but in insufficient quantities, by the atmosphere. But M. Risler repeats with great care the experiments of de Saussure, and confirms the accuracy of his conclusions. Hyacinths and other bulbs were placed with their roots suspended in water colored with soluble extract of humus. When these plants were placed in the sun, the water became rapidly decolorized. Other roots such as carrots, also germinating grains of wheat, were observed to produce the same effects. An extract of humus was exposed at a somewhat elevated temperature to sunlight under a bell glass. Microscopic plants developed in great abundance. As long as these plants continued to develop the infusion was transparent and did not putrefy in the slightest degree: and yet there was a constant evolution of $CO_2$ as shown by analysis of the air in the bell-glass. "Now the cellules formed in the liquid contained carbon. This carbon did not come from the $CO_2$ of the air, for the liquid, far from *absorbing*, disengaged $CO_2$. Therefore the soluble humus must have furnished the carbon *directly* to the vegetable cells." It could not have furnished it *indirectly* in the form of $CO_2$ derived from decomposition of the organic

* Carpenter, Comp. Phys., p. 285.

† Bib. Un. Arch. des Science, new period, vol. 1, p. 305.

matter, otherwise *oxygen* instead of $CO_2$ would have been eliminated. M. Risler thinks moreover that the *embryo* in germination takes up soluble organic matter in the form of humus in addition to the soluble organic matter contained within the cotyledons, and that the evolution of $CO_2$ by germinating seeds is due in part also to the oxydation of humus. Finally, according to the same author, the formation of roots in all plants, but particularly those containing much starch or sugar, is due to the direct absorption of humus, and not, as is generally supposed, by the fixation of carbon by means of light. "In order," says he, "that $CO_2$ of the air should form these substances, it is necessary, in the beet and the potato, that there should be a descending sap, which there is not." Moreover if the carbon was taken from the soil in the form of $CO_2$ there should be elimination of oxygen instead of evolution of $CO_2$; but the converse is the fact as has been proved in the most indisputable manner by de Saussure and Boussingault.*

Mülder is equally explicit in affirming that plants absorb soluble organic matter which is converted in the roots, by elimination of a portion of the carbon, into starch and sugar. — Mülder, pp. 620, 664, 682. Thus according to these authors, *sap is actually elaborated by the roots* from organic manures.

Now according to the theory which I propose, *this change from humus into starch, sugar, or cellulose, furnishes an additional life-force.* Humus is a more highly carbonized substance than either starch or cellulose. By the *partial decomposition of humus* in the tissues of the plant, with the elimination of a portion of its carbon (removed by oxydation), *a chemical force is set free which serves to assimilate the remainder.* Hence this process of evolution of $CO_2$, as we have already said, is opposed by light but favored by darkness and heat. Light favors the formation of chlorophyll, of woody fibre, of essential oils, gums, etc.; darkness, heat, and organic manures favor the formation of sugar, starch, etc. Hence the explanation of the well-known fact that by covering up the lower portions of potato plants by heaping earth around them, many buds which would otherwise form leafy *branches* develop into *tubers.* Hence also the explanation of the equally well-known fact that the roots of plants seek and grow most rapidly in the

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*Bib. Un. Arch. des Sciences, new series, vol. 1, p. 5.

direction of most abundant food. If the sap is elaborated entirely in the leaves, it is difficult to understand why the descending sap should flow in greater abundance in one direction than another. But if sap is elaborated in *the root itself* it is easy to see why growth is most rapid in the direction of most abundant manure. It is easy to see, too, why roots avoid the light; since light decomposes $CO_2$, and therefore must be unfavorable to the formation of this substance.

7th. It is a well-known fact that the so-called *respiration of plants* consists of two distinct and apparently opposite processes, 1st, the absorption of $CO_2$ by the leaves and also in solution by the roots, the decomposition of this $CO_2$ by means of light with the fixation of the carbon and the elimination of the oxygen; 2d, the recomposition and evolution of $CO_2$. The *decomposition* of $CO_2$ undoubtedly takes place in the leaves, but where the recomposition of $CO_2$ takes place is not so well ascertained. It is exhaled, however, like the oxygen, from the leaves. The process of decomposition of $CO_2$ takes place only during the day, as light is absolutely necessary for this process. The *recomposition* of $CO_2$ takes place night and day, although its exhalation, according to some observers, seems to be more abundant during the night. The process of decomposition of $CO_2$ is well understood — of that of recomposition our knowledge is very imperfect. M. Rissler's explanation of this latter process seems most probable. Plants, we have seen, undoubtedly absorb soluble organic matter, i. e. humus. Humus we know is a more highly carbonized substance than cellulose or starch. This humus is therefore oxydized in the roots and interior of the trunk, away from light, by means of oxygen also absorbed by the roots and thus forms $CO_2$. This $CO_2$ then circulates in the sap to be exhaled by the leaves or perhaps to be again decomposed by sunlight in this organ. In the absence of light the whole is exhaled undecomposed. This readily accounts for the apparently greater exhalation of $CO_2$ during the night. A series of well-conducted experiments would test the truth of this view. If it is true, there should be a relation between the richness of the soil in organic manures and the amount of $CO_2$ exhaled. For a given amount of growth, the amount of $CO_2$ exhaled is the measure of the amount of food taken up in the form of organic matter, and the amount of oxygen exhaled is the measure of the amount of food taken in the form of mineral matter. Or if the exhaled $CO_2$ is decomposed in the leaves during the day, then of course

the difference between the amount exhaled during the night and day would enter as an element in the calculation. Also it would seem that those plants, especially, which frequent rich shady spots, should exhale proportionally more $CO_2$ and less oxygen, than those loving thin soils and sunny places.

In plants, then, there are *two sources of organizing force*, the relative proportion of which varies infinitely, according to the amount of light, heat, color of the plant, and richness of the soil in organic matters. The two sources are *immediately*, 1st, the decomposition of $CO_2$; 2d, the decomposition of soluble highly carbonized organic matter; *remotely* the two sources are *light* and *heat*. In plants which first take possession of desert spots, bare rocks, etc., the *first* is the only source. In pale plants and fungi the *second* is the only source; but in most plants the two are combined in various proportions. The 1st must of course be considered the most fundamental and necessary, the 2d being evidently supplementary. The decomposition of $CO_2$ by sunlight may be considered as the original source of all vegetation, but in most of the higher orders of plants the process of nutrition is expedited by the reabsorption of organic matter before it again returns to the condition of $CO_2$, HO, and $NH_3$.

8th. The *egg* during incubation, absorbs oxygen, evolves $CO_2$, and probably HO, and loses weight. As the result of this evolution of $CO_2$ we find the egg *develops*. What it *loses in weight* it *gains in organization*. Now what is the source of the organizing force? It evidently bears a direct relation to the loss of weight. Here also, then, we have *partial decomposition furnishing the necessary force*. A portion of the organic matter, falling from the organic to the mineral plane, sets free a force which raises the remaining portion into a slightly higher condition. Heat is evidently the physical force or agent which is transformed, not directly, but *indirectly, through chemical affinity*, into vital force. In other words, heat is the agent which effects the necessary decomposition. The phenomena of development of the egg is, therefore, very similar to that of the seed.

9th. *After the hatching* of the egg, the animal no longer loses weight; because recomposition of food taken *ab externo* proceeds *pari passu* with decomposition. But in this case also *decomposition supplies the force by which recomposition is effected*, and growth and development

carried on. As this is an important point I will attempt to explain it more fully.

It is well known that in the animal body there are, going on constantly, two distinct and apparently opposite processes, namely, decomposition and recomposition of the tissues; and that the energy of the life is exactly in proportion to the rapidity of these processes. Now according to the ordinary view, the animal body must be looked upon as the scene of continual strife between antagonistic forces, chemical and vital; the former constantly tearing down and destroying, the latter as constantly building up and repairing the breach. In this unnatural warfare the chemical forces are constantly victorious, so that the vital forces are driven to the necessity of contenting themselves with the simple work of reparation. As cell after cell is destroyed by chemical forces, others are put in their place by vital forces, until finally the vital forces give up the unequal contest, and death is the result. I do not know if this view is held by the best scientific minds at the present day, as a fact, but it certainly is generally regarded as the most convenient method of representing all the phenomena of animal life, and as such has passed into the best literature of the age. Certain it is, however, that the usual belief, even among the best physiologists, is that the animal tissue is in a state of unstable equilibrium; that constant decomposition is the result of this instability, and that this *decomposition and this alone, creates the necessity of recomposition* — in other words, creates the necessity of food. But according to the view which I now propose, decomposition is *necessary to develop the force* by which organization of food or nutrition is affected, and by which the various purely animal functions of the body are carried on — that decomposition not only creates the *necessity* but at the same time furnishes the *force* of recomposition.

But it will no doubt be objected that according to the principles of conservation of force, decomposition of a given amount of matter can only effect the recomposition of an equal amount — that a given quantity of matter falling a given height, can only raise an equal quantity an equal height. The whole force developed by decomposition seems to be expended in maintaining the body at a given position. How then can *growth* and *animal activity* go on? The answer to this question is obvious enough when we recollect the nature of the food of animals.

Animals, it is well known, cannot feed upon mineral matter, but only on food already organized, at least up to the vegetable condition. But when decomposition takes place, the animal matter returns no longer to the vegetable condition from which it was immediately raised, but to the mineral condition. It is decomposed into $CO_2$ HO, and *urea*. This last substance, though not strictly a mineral substance, is far below the condition of vegetable matter. Thus it is evident that *a given quantity of matter falling down from the condition of animal to that of mineral matter*, i. e. from the 4th to the 2d plane, *would develop force sufficient to lift a larger quantity of matter from the vegetable to the animal condition*, i. e. from the 3d to the 4th plane, and yet perhaps leave much residual force unexpended. Thus it is possible, and not only possible but certain, on the principle of conservation of force, that decomposition of animal tissues should set free a force, a part of which is consumed in the recomposition of a *larger* amount of new matter, and thus maintaining growth; a part in animal heat and a part in animal activity of all sorts. In this view of the case we see at once the absolute necessity that the food of animals should be organized. Upon the principle of conservation of force, growth, and animal activity, in a word, animal life, would otherwise be impossible.

It follows also, from the above, that the higher the organization of the food the smaller the amount of force necessary to effect assimilation, and therefore the larger the amount of residual force to be expended in animal heat and animal activity. In this we find a ready explanation of the superior activity of *carnivorous* animals, and the loss of animal activity which results in a state of domestication from the use of vegetable diet: also of the supposed superior activity of men fed upon meat diet.

10th. I have spoken thus far of only one source of vital force in animals, namely, the *decomposition of the tissues*. I have attempted to show how, upon the principle of conservation of force, this is sufficient to carry on the growth and the activity of the animal organism. But decomposition of the tissues, though the fundamental source — the source characteristic of and peculiar to animals — of immediate and universal necessity in this kingdom, and in many cases sufficient of itself, is not the only source. There is also in animals, as in plants, a supplemental source, namely, *the decomposition of food*.

It is well known that the food of animals consists of two kinds, the

nitrogenous, such as albumen, fibrine, caseine, etc., and the non-nitrogenous, such as fat, starch, sugar, gum, etc. According to all physiologists since Liebig, the nitrogenous alone are used in the repair and growth of the tissues. The non-nitrogenous are either quickly consumed in respiration, or else are laid up in the form of fat for future consumption in the same way. Now there can be no doubt that animals may live entirely on nitrogenous food; in which case the whole vital force, whether for assimilation of food or for animal heat and animal activity, is derived from the decomposition of the tissues. This is the case also, apparently, in the starving animal, particularly if lean. But in almost all cases much food in the form of fat, starch, sugar, etc. (non-nitrogenous), is never transformed at all into tissues, but is taken into the blood, gradually decomposed, oxydized in the course of the circulation, changed into $CO_2$ and HO, and finally removed by exhalation from the lungs. Now what is the object of the non-nitrogenous food, since these do not form any part of the tissues, but are again decomposed and thrown out of the system? The answer usually given is, that such food is used in the animal economy solely as fuel to keep up the animal heat. On this view it is difficult to see why this class of food should be used at all, especially in warm climates. But according to the view which I propose, we have here an *additional source of vital force.* The decomposition of these ternary compounds sets free a force which is used in organizing and assimilating other matter (nitrogenous) and in producing animal activity and animal heat. As in plants, although the decomposition of $CO_2$ by sunlight is all that is absolutely necessary for growth and development, yet the decomposition of organic food supplies an additional force which greatly increases the vigor and rapidity of vegetation; so in animals, although *decomposition of the tissues* is all that is absolutely necessary to furnish the force of growth and the phenomena of animal life generally, yet the decomposition of non-nitrogenous organic *food* furnishes additional force by which growth and animal activity may be maintained without too great expenditure of the tissues.

11th. In what, then, consists the essential difference between animals and plants? There can be no doubt that it consists, generally, in their relations to one another and to the mineral kingdom. Plants occupy a middle ground between the mineral and animal kingdom — a necessary halting place for matter in its upward struggles. But when we attempt

to define this relation more accurately, the problem becomes much more difficult. It is indeed probable that no single distinction will be found free from objection. The commonly received, and, to a certain extent, very correct idea is, that the essential distinction consists in their relation to $CO_2$. Plants *de*compose and animals *re*compose $CO_2$. The beautiful manner in which the two kingdoms stand related to each other through these converse processes, is familiar to all. But it is well known that most plants carry on both of these processes at the same time, while some, as fungi, pale plants, &c., only recompose $CO_2$ like animals. It seems to me that at least an equally good fundamental distinction may be found in this, that in plants the fundamental and necessary source of vital force is the decomposition of its *mineral food;* while in animals the fundamental source of vital force is the decomposition of its *tissues*. It is true that in what I have called the supplementary source of vital force they seem to meet on common ground, namely, the decomposition of *organic food;* but even here there is this essential difference, that in plants this decomposition of organic food is only partial, and therefore furnishes not only *force* but *material* for organization; while in animals the decomposition is complete and therefore furnishes only *force*.

As a necessary result of the above, it would seem that the "*vortex*" of Cuvier is characteristic of animals. There seems no reason to believe that a tissue once formed in plants is ever decomposed and regenerated, as is the case in animals. When plant-cells decompose, the tissue dies. Hence the absolute necessity of *continuous growth* in plants. In this kingdom *life* is synonymous with growth. There is no possibility of life without growth. There is no such thing as determinate size, shape, or duration. There is no such thing as maturity, or if so, death takes place at the same instant. As cell life is necessarily of short duration, and as there is no regeneration of tissues in plants, it is evident that the life of the tissues must be equally short. Thus plant life can only be maintained by the continual formation of *new tissue* and a constant travelling of the vital force from the old to the new. In exogenous plants the direction of travel is from the interior to the exterior; in endogens from exterior to interior, and still more from below upwards by the continual addition of new matter at the apex. In fungi where there is no such superposition of new tissue upon the old, where growth takes place by multiplication of cells throughout the

whole plant — in other words, a true interstitial growth as in animals — since there is no regeneration of tissues, the duration of the life of the plant is limited by the duration of cell-life.

The *respiration* of animals, also, differs essentially from that of plants. At one time the absorption of $CO_2$ and exhalation of O was called the respiration of plants. It is universally admitted now, however, that this is rather a process of assimilation than of respiration. The recomposition and exhalation of $CO_2$, as soon as discovered, was very naturally likened to animal respiration, and is in fact looked upon by many, as for example the physiologist Carpenter, as a true respiration. But there is an essential difference between this and animal respiration, which I have already pointed out. Its very significance is radically different. The essential object of animal respiration is the removal of poisonous decomposed matters from the organism. The so-called respiration of plants, on the contrary, is rather a process of assimilation, since by it the too highly carbonized organic food, by the elimination of a portion of its carbon, is brought into a proper condition for organization. A true respiration is necessarily connected with a change of the matter of the tissues — with the vortex of Cuvier — which has never been shown to exist in plants. It is true the exhalation of $CO_2$ has been looked upon by some physiologists as indicative of a regeneration of tissues, but I have already shown that this is probably not the case, but on the contrary that the $CO_2$ is formed by the partial decomposition of highly carbonized organic *food.*

12th. The most natural condition of matter is evidently that of chemical compounds, i. e. the mineral kingdom. Matter separated from *force* would exist, of course, only as elementary matter or on the *first plane;* but united with force, it is thereby raised into the *second plane* and continues to exist most naturally there. The *third plane* is supplied from the second, and the fourth from the third. Thus it is evident that the quantity of matter is greatest on the second and least on the fourth plane. Thus nature may be likened to a pyramid, of which the mineral kingdom forms the base and the animal kingdom the apex. The absolute necessity of this arrangement on the principle of the conservation of force may be thus expressed. *Matter, force,* and *energy,* are related to one another in physical and organic science somewhat in the same manner as *matter, velocity,* and *momentum* in mechanics. The whole *energy* remaining constant, the greater the *intensity* of the force

(the elevation in the scale of existence) the less the quantity of matter. Thus necessarily results what I have called the pyramid of nature, upon which organic forces work *upwards* and physical and chemical forces *downwards.*

13th. As the matter of organisms is not created by them, but is only so much matter withdrawn, borrowed as it were, from the common fund of matter, to be restored at death; so also organic forces cannot be *created* by organisms, but must be regarded as so much force abstracted from the common fund of *force*, to be again restored, the whole of it, at death.* If, then, vital force is only transformed physical force, is it not possible, it will be asked, that physical forces may generate organisms *de novo?* Do not the views presented above support the doctrines of "equivocal generation" and of the original creation of species by physical forces? I answer that the question of the origination of species is left exactly where it was found, and where it must always remain, namely, utterly beyond the limits of human science. But although we can never hope by the light of science to know *how* organisms originated, still all that we do know of the laws of the organic and inorganic world seem to negative the idea that physical or chemical forces acting upon inorganic matter can produce them. Vital force is transformed physical force, true, but the necessary *medium* of this transformation is an organized fabric; the necessary condition of the existence of vital force is therefore the previous existence of an organism. As the existence of physical forces cannot even be conceived without the previous existence of matter as its necessary *substratum*, so the existence of vital force is inconceivable without the previous existence of an organized structure as its necessary substratum. In the words of Dr. Carpenter, "It is the speciality of the material substratum thus furnishing the medium or instrument of the metamorphosis which establishes and must ever maintain a well-marked boundary line between physical and vital forces. Starting with the abstract notion of force as emanating at once from the Divine will; we might say that this force operating through inorganic matter, manifests itself as electricity, magnetism, light, heat, chemical affinity, and mechanical motion; but when directed through organized structures, it effects the operations of growth, development, and chemico-vital transformations."

* Carpenter, Phil. Trans., 1850, p. 755.

## 2. Vibrations in the Waterfall at Holyoke, Mass. By Prof. E. S. Snell, of Amherst, Mass.

At the meeting of the American Scientific Association held in Montreal, August, 1857, I read a paper on the vibrations of the fall at Holyoke, in which I attributed the movement to the rarefaction of air in the tube behind the sheet, this rarefaction being caused by the action of the water, which carries down the adjacent air, and throws it up in foam mostly on the outside of the fall. In that paper I described two modes of vibration which I had observed, that agreed well with the supposition of acoustic pulsations in a tube of air 1,008 feet long, and having *two* nodal sections in one case, and *four* in the other. I also stated my impression that I had, many years before, noticed a much slower rate of vibration, which accorded equally well with the existence of *one* nodal section in the tube.

Since the reading of the above-mentioned paper, I have observed the condition of the fall at four different times. In October and November, 1857, I noticed the same rates of vibration, very nearly, which I had previously reported. But on the 16th of April, 1859, I found the water four or five feet deep on the edge of the dam, the temperature of the air about 45°, and the number of oscillations only *eighty-two* per minute. Again, on the 25th of July last, I found the water lower than I had seen it before (less than three inches deep), and no vibrations, either in the sheet, or the air at the end of the cavity behind it.

There are, therefore, at least *three* very different rates of vibration in this fall, the slowest when the depth of water is greatest, and the most rapid when it is about one foot deep, the vibrations ceasing altogether when the depth is so small as three inches. In the following tabular statement, the four first columns show at once the facts as they stand connected in the few observations which I have made, and the last column contains the numbers calculated for an open tube 1,008 feet long, with *four* nodes for the *third* and *fourth* observations, *two* nodes for the *first*, *second*, and *fifth*, and *one* node for the *sixth*.

| | Time of observation. | Temperature of air. | Depth on dam. | Observed No. vibrations per min. | Calculated No. vibr. per min. |
|---|---|---|---|---|---|
| 1 | July 25, 1857, | 80° | 2 feet | 137 | 136 |
| 2 | July 29, " | 75 | 2 " | 136 | 136 |
| 3 | Aug. 6, " | 75 | 1 " | 257 | 271 |
| 4 | Oct. 7, " | 65 | 1 " | 258 | 268 |
| 5 | Nov. 24, " | 30 | 2 " | 140 | 129 |
| 6 | Apr. 16, 1859, | 45 | 5 " | 82 | 66 |
| 7 | July 25, 1859, | 70 | 3 inches | none. | none. |

I used the formula in Peirce's Treatise on Sound, $N = n\frac{V}{L}$, where N is the number of vibrations, $n$ the number of nodes, V the velocity of sound, and L the length of the tube. It is observable, that the calculated rates are higher than the observed, in the cases of most rapid vibration, and lower in those of least rapidity, while in the medium rates they very closely agree. As to the seventh case, the sheet was so thin, that it was divided into filaments and broken into spray, and the air had free ingress and egress through its whole length; the acoustic tube being thus destroyed, no vibrations could be produced.

Notwithstanding the discrepancies between the numbers in the two last columns, I think the general correspondence between them points to the true nature of the cause, especially when taken in connection with the fact, that the pulsations are noticeable only in the water and in the air, — not at all in the dam itself, nor in the rock or soil immediately adjacent. It must be remembered also, that the pitch of musical pipes does not fully conform to the formulæ, but varies with the breadth of opening and the mode of exciting vibrations.

This seems to be one of the numerous cases, in which the body which excites vibrations in another, is itself thrown into synchronous vibration by reaction, and then, by its own inertia or elasticity, controls the common rate of both. The sheet of water in its descent first produces rarefaction of the enclosed air by removing a part of it. The immediate effect is a collapse of the sheet of water, as well as a rush of air in at the ends. But the inertia of a thick mass of water will prevent its recovering its natural position so soon as if it were thinner; hence, the air-column divides itself into such a number of segments, that the water and the air can adjust their movements to each other. In a manner somewhat like this, a stream of air from the lips, driven across

the embouchure of a flute, excites vibrations in the column of air, with such frequency that it can itself vibrate in unison with it. But, if the stream is blown more and more swiftly, its elasticity will at length be too great for so slow a rate, and then the column will divide into shorter segments, and the two will continue their vibrations harmoniously upon a higher key. A skilful player can in this way, by his mere breath, produce *six* or *eight* harmonic notes on the flute, when all the holes are closed.

At the time when I witnessed the comparatively slow oscillation of eighty-two per minute, I was surprised by the great strength of the current of air, as it rushed into the opening at the end of the dam. I could not venture within the passage through the pier, lest I should be swept in behind the sheet; nor could I stand at the entrance of the arch, without bracing myself, by placing both hands on the corners. There was, however, no alternate *outward* blast, but only a lull, or cessation of all motion; which shows, that the excess of air that pours in at every pulse, is carried out again in some other way; and there is no conceivable way for it to escape, except to be driven down by the falling water, and poured up externally in a bed of foam. It had never occurred to me before, that the velocity of the air-current must be greater the longer the interval between the pulses, since the rarefaction within the tube will be greater nearly in the ratio of the same interval.

In September, 1857, a paper was read before the Boston Society of Natural History, in which objections were made to my view of the source of the vibrations, and the cause assigned for them was the impulse on the rock produced by successive swells of the sheet, extending parallel to the edge of the dam, from one side of the river to the other. If this is the cause, then the vibrations are first excited in the rock and communicated thence to the air. But the *rock* and *soil* in the immediate vicinity of the Holyoke fall are not perceived to move in the least, while the *air* sways a loose garment back and forth three or four inches, keeping time with the visible and audible pulsations of the sheet of water, and at the end of the tube sometimes rushes so violently, that a man can scarcely stand against it. That alternate swells and contractions cannot exist in a falling sheet of water, and if so, that they cannot cause sensible undulations in the earth, I am not prepared to assert; but I believe that any unbiased observer will find it quite absurd to apply such an explanation to the strong puffing of the air which is usually so noticeable at the Holyoke fall.

3. NOTE ON THE DISCHARGE OF ATMOSPHERIC ELECTRICITY THROUGH GAS MAINS. By Prof. B. SILLIMAN, Jr., of New Haven, Conn.

DURING the past year two cases have fallen under my observation of the discharge of atmospheric electricity through the iron gas mains of New Haven, with effects which, so far as I know, have not been previously noticed.

*First case.* On the last Sunday of June, 1858, about 4 P. M., a violent thunderbolt fell on the spire of the Wooster Place Baptist church in New Haven, and was conveyed without injury to the spire (227 feet high) to a point less than twenty feet from the earth. At this point, owing, as was afterwards ascertained, to an imperfect ground termination of the conductor, the electric tension was sufficient to produce a lateral disruptive discharge through a wall of brick nearly twenty inches in thickness, to meet a gas pipe on the inside of the wall directly opposite to the lightning conductor. By the new channel thus forcibly gained, the electric discharge was conducted to the main pipes of distribution, and no further immediate effects were seen. Within a few days after the discharge, however, the rapid escape of gas on the street in front of the church was noticed, as well by the odor as by the sickly condition and subsequent death of the shade trees lining the street. Upon opening the ground it was found to be saturated with gas, and every joint in the whole length of the street (some forty in number in a six inch pipe) was discovered to be leaking profusely.

The inference seemed unavoidable that the leakage was occasioned by the electrical discharge.

*Second case.* During the last week in July, 1859, another very energetic discharge of atmospheric electricity fell upon a house in George street, New Haven. This house was supplied with gas, and while but little injury was done to the dwelling, and none at all to its inhabitants, the gas mains in the whole street, to the number of over sixty joints, were found to be entirely loosened. They all required to be repacked.

In both these cases the loosened joints were confined to the line of pipe in the street fronting the buildings, and the injury appeared to be arrested by the abutting pipes of the next cross street.

In June of this year the same church spire struck in 1858 was again the subject of a second accident of this sort. The wall of brick was

again perforated near the same place and in the same manner as last year, with the additional circumstance that the gas pipe in the church was fused or burnt off at the point of contact of the escaping discharge, and the gas being thus set on fire, in its turn set fire to the wainscoting or furring behind which the pipe ran. But either because the volume of the discharge was less than last year, or because a portion of it found a lateral escape, there was no effect produced in disturbing the joints of the street mains.

The mechanical effects herein described may be due to a sudden and almost explosive expansion of the gas in the mains at the point of electrical discharge, notwithstanding the enormous extent of the metallic conductors. It is difficult, however, to reconcile the facts with existing views of electrical tension and discharge, and they are now brought before this Association with the hope of eliciting a satisfactory explanation of them.

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4. On the Means of Preventing the Alteration of Metallic Surfaces employed to Close and Break a Voltaic Circuit. By Pres. F. A. P. Barnard, of Mississippi.

It is well known that, while a dynamic electrical current will circulate for any length of time through an unbroken metallic conductor sufficiently large to convey it without a sensible elevation of temperature, without injury, the effect of repeatedly interrupting the current at the same point is gradually destructive to the substances between which the interruption occurs. In the case of very powerful currents, this destructive effect becomes soon perceptible. But even in that of the most feeble, it is not any the less sure, though more slowly manifested. Nor, so far as is yet known, is there any substance, however unalterable under ordinary exposures, which is capable of entirely resisting it. Platinum contacts conveying an energetic current exhibit very remarkable changes, after a brief period of use. In a powerful Ritchie induction machine belonging to the University of Mississippi, in which contact is made between a hammer and anvil of platinum, the surfaces in contact being about $\frac{2}{10}$ of an inch in diameter, these surfaces are roughened, as if they had been corroded by some active chemical reagent, and the edges present a burr, like what might be produced by a blow

while the metal is partially softened. Yet these surfaces come together without percussion, and it is only the separation which is sudden.

When currents, as in this case, are strong, the alteration of the metals does not very sensibly impair their conducting power. In the case of feeble currents, it is otherwise. The surfaces become gradually discolored, and a coating of imperfect conducting power is formed over them, which resists the flow of the current and at length arrests the action of the apparatus it is designed to operate.* For some of the uses to which electro-magnetism is applied, this is a very serious evil. Chronometers, chronographs, and some descriptions of telegraph, depend for their accuracy upon the invariable passage of the current. It is, therefore, desirable to find some means to prevent, if possible, the injurious alteration of the surfaces of contact, which has just been described.

The attention of the writer was seriously directed to this subject, by observation of the performance of an electric clock constructed for the University of Mississippi, of which a description was presented to the Association at the meeting in Baltimore. The clock was completed and set in motion by the constructer, in the month of May, 1858; but owing to delays in the preparation for use of the building for which it was designed, it was not until January, 1859, that it could be set up at the University. From that time until the present it has been running; with such interruptions as were occasioned by the prosecution of the experiments, of which it is the object of this paper to state the results.

The batteries employed were Seaver's water and sulphate of copper batteries, exposing each considerably less than one quarter of a square foot of surface of zinc. At first, three of these elements were used in connection; afterwards only two; and finally a single one. One seems to be sufficient; but for excess of security two are now commonly employed.

The rupture of the electric circuit took place between an angular

* These spots, in the case of platinum, resist all chemical reagents which do not attack the metal itself. Boiling nitric, hydrochloric, or sulphuric acid produces no change in them. Exposed, however, to the heat of the blow-pipe, they become dull white; and afterwards, under the burnisher, entirely disappear. They consist, therefore, of platinum in the state of minute division, called platinum-black. For this examination I am indebted to Dr. E. W. Hilgard, State Geologist of Mississippi.

edge of platinum, and a plane surface of platinum foil. Whenever action ceased, it was always found sufficient to move the foil so far as to present a fresh surface of contact, in order to reëstablish it; the opposing edge remaining constantly the same. To prevent the stopping of the clock, it seemed, therefore, to be sufficient to replace the foil from time to time by a new one. But this process itself implied a momentary stopping, unavoidable except by introducing double, or collateral, contacts, for which no arrangement had been made in the original construction.

The first mode which occurred of obviating the difficulty was to connect the two electrodes between which the separation took place, with the opposite plates of a condenser. The injurious effect being manifestly a consequence of the dispersive, or explosive, action of secondary currents, it was believed that if the visible spark could be entirely suppressed, the electrodes would remain unaffected.

For greater convenience, a series of experiments was instituted on contacts outside of the clock, by means of an apparatus kept in motion however by the clock circuit, and breaking electric contact at every second. Copper, iron, and platinum, dipping into mercury, were successively employed, and also platinum against platinum. The result corresponded substantially with the anticipation. A mercury cup which, without the condenser, would, in twenty-four hours, become thickly covered with a black powder — which powder was also scattered about beyond the cup — continued, with a condenser attached, to remain for many weeks, with scarcely a tarnish. And a slip of platinum foil, after long serving in the same way, exhibited only the effect of mechanical action upon its surface.

The condensers employed in these experiments were at first constructed of sheets of tin foil, each presenting a little more than a square foot of surface, separated by an interposed sheet of tissue paper. After the lapse of about a week, one of these condensers was found to transmit the primary as well as the secondary current; and accordingly, in subsequent constructions, the thinnest description of foreign letter post was substituted for the tissue paper.

The tissue paper which failed, did not exhibit, on the closest examination, any trace of alteration by the electricity; but the sheet which had been employed, and also those which had not been so, presented many minute apertures, as well as much irregularity of thickness; and it was presumed that the pressure which had been applied to bring the

surfaces of metal near to each other, had at length established a contact through some one of these imperfect points.

A more convenient form of construction has been since found to be to cut the tin foil into small sheets of about three by four inches, interposing between them the insulating paper, and connecting the alternate sheets with the opposite electrodes. The spark, which now no longer appears at the point of rupture of the current, discharges itself in the condenser with a sound, which, even with the feeble batteries above described, is audible at some distance. It was at first apprehended that this constant repetition of an explosion, however slight might be the effect produced at each recurrence, would at length break down the insulation at some point, and so permit the current to pass. Accordingly some expedients were at first resorted to, in order to avert this danger.

One of the first of these consisted in connecting the oppositely charged leaves of the condenser with two small metal plates insulated from each other; and then, by means of a pointed screw of fine thread passing through one of them and directed toward the other, causing the discharge to take place visibly outside of the condenser. This method was perfectly successful when the insulating papers were made somewhat thick; by employing, instead of the letter post, several thicknesses of post-office wrapping paper between the tin foils; but with the letter post alone it was very difficult to bring the point of the discharging screw near enough to the opposite plate, without establishing actual contact; and also, when this end had been gained, it was difficult to prevent its being lost again presently, by the change of figure of the apparatus.

A second mode of effecting the same purpose, was to make a condenser with insulators consisting of single layers of post-office wrapping paper, and connecting with this a second very small one, consisting of a single pair of tin foils insulated by letter post. It was supposed that the discharge would always take place through the thinner insulator; and that after this had been exposed as long as should be thought safe, it might easily be renewed without disturbing the rest. Experience has, however, taught that the danger which these expedients were designed to obviate, does not exist. Condensers which have been in use for many months still continue to perform as well as at first; and the examination of the letter post insulators, after this long exposure, shows but doubtful marks of the action of the electricity. There are, in-

deed, here and there minute, and as it were, needle-point, perforations; but it is impossible to say that these were originally made by the discharge.

The effect of the condenser in suppressing the spark is dependent both upon the extent of its surface, and upon the thinness of the insulation. The low battery power with which these experiments have been conducted, requires not so much as one square foot of tin foil in connection with each electrode, to produce all the effect desired. More energetic arrangements, with powerful electro-magnets in the circuit, require much greater surface.

The application of the condenser to a voltaic circuit seems first to have been made, or at least recommended, by Fizeau, in the year 1853. His plan was to employ varnished metallic plates presenting two or three square feet of surface. Ruhmkorff introduced the condenser into his machine, in the form of large sheets of tin foil separated by oiled silk, each sheet having a surface of seven or eight square feet. Poggendorff, however, in 1855, expressed the opinion, founded on some experiments of Halske, confirmed by himself, that, to the Ruhmkorff machine as then constructed, a pair of tin foils of the size of an octavo page, separated by thin paper, were sufficient for the purpose for which the condenser had been introduced, namely, that of exalting to the maximum the intensity of the secondary current in the external helix.

The object at that time aimed at having been merely to promote the activity of the Ruhmkorff machine, little attention appears to have been paid to the question to what extent the condenser might be made effectual in suppressing the secondary spark at the break of the primary circuit. The diminution of the spark was noticed, and also the diminution of its destructive effect; but these observations seem to have suggested no further investigation in that direction. It is believed, nevertheless, though the subject requires still further examination, that, by sufficiently enlarging the surface of the condenser, and diminishing the thickness of the insulation, every trace of the spark may be removed from the strongest battery current.

The Ritchie induction machine of the University of Mississippi is capable of giving a spark from the external helix, of nearly thirteen inches long. The exciting battery consists of three Bunsen's elements, each exposing about three square feet of zinc plate (estimating both the surfaces exposed) and the spark at the break of the primary, when

the condenser is not connected, is like a brilliant flame. This is greatly reduced by attaching the condenser of the machine itself, but continues still to be very bright. An additional condenser containing fifty-six square feet of tin foil (twenty-eight square feet on each side) so far suppresses it, that it only appears as an occasional scintillation.* In this additional condenser, the insulation is by means of post-office wrapping paper, which is much too thick, and the whole is rolled upon a wooden cylinder altogether too loosely to secure proper compression.

The theory of the effect of the condenser in the Ruhmkorff machine is not a matter involved in the present inquiry. Inasmuch, however, as it seems nowhere (so far as the writer has observed) to have been very distinctly stated, it may be permitted to say here, that the inferior intensity of the spark in the external helix, when the condenser is not attached, appears to be a consequence of electro-static induction from the secondary developed in the internal helix. This latter secondary is drawn off by the condenser, and its repressive influence upon the outer current is thus diminished or destroyed. This explanation also serves to account for the well-known fact that when the current from the outer helix is sent through a vacuum, it is of no consequence whether a condenser is employed or not; for as the repulsion exerted by the inner secondary current tends simply to expel the similar electricity from the outer helix, and so equally to drive it either way (forward or backward), it drives it forward when there is no resistance, or when there is but a feeble resistance (as in vacuo), and backward when there is a resistance.

The experiments above detailed rendered it evident that, by means of a properly constructed condenser, the surfaces of electric contact in the clock might be preserved from injurious alteration. For it may be here observed that, with incombustible metals and with currents of low intensity, the initial secondary produces no sensible change. It was not, however, exactly convenient to place the condenser in the clock; and hence it became a subject of inquiry how far its protection might be relied on when removed to a distance.

Leading wires were at first connected with the wires of the circuit

* It is proper to say that, at the time this experiment was made, the battery was not in its highest state of activity. The experiments with the Ruhmkorff machine connected with this inquiry, are still in progress.

on each side of the point of rupture, and extended to the condenser at different distances, from a few feet to fifty or a hundred. It was found that, for moderate distances, the performance was sufficiently satisfactory; but that when, either from increase of distance or reduction of the size of the leading wire, the resistance was considerably increased, the efficacy of the condenser was but partial.

It was further found not by any means to be important that the connections of the condenser with the circuit should be near the point of rupture. Suppose, for instance, that an intensity magnet is operated by a battery at a distance of thirty or forty feet, and that the rupture of circuit takes place near the battery; the condenser is perfectly effectual if attached to the opposite extremities of the helical envelope of the magnet. Moreover, in any circuit in which the rupture is near the battery, it is rather advantageous than otherwise, to make the connections with the condenser in such a manner as to throw the battery out. If, for example, the break occurs near the positive end of the battery, it is best to attach one of the leaders of the condenser to the circuit wire beyond the break, and the other to the negative extremity of the battery instead of the positive.

In the Ruhmkorff coil, as the object to be secured is the discharge of the secondary current of the inner helix, the points of attachment of the condenser theoretically best, are the extremities of that helix itself.

While these experiments were in progress, the question naturally presented itself whether, in a piece of mechanism moved by electricity, like the clock, there is any necessity that the current should break within the apparatus at all or not? Supposing the electric contact to be made within the mechanism and broken without, it is evident that the condition of the surfaces of contact where the alteration is liable to occur, could be conveniently watched, and by mechanical means easy to devise, prevented from ever reaching a condition which should disturb the action of the apparatus. This idea was followed out to a result entirely satisfactory.

In order to accomplish the object desired, it became necessary to alter, in some respects, the electric connections as described in the account given of the clock in the published proceedings of the Baltimore meeting of the Association. By reference to that account it will be seen that the current, passing through the impulse weights which maintain

the motion of the pendulum, actuates directly the magnets in the clock. In the modification given to the plan, this current was made to actuate two magnets in a separate little piece of apparatus, called a *break-circuit*, by means of which this original current was broken, while a second current was made to actuate the magnets of the clock. This contrivance having been added, there no longer remained the slightest trace of electric light in the clock; and by applying condensers to the break circuit, the spark was also entirely suppressed there also. In this condition the clock has now been running since some time in May last, without the appearance of any sensible alteration in any part of the apparatus.

Incidentally, however, in the prosecution of experiments suggested by this investigation, a very simple mechanical method of obviating the difficulty which gave rise to the inquiry, has presented itself. It is merely to make every contact *double* between two firm points on one side, and two independent elastic springs on the other. Then to adjust these so that one of them may invariably break an instant before the other. The secondary spark will always occur at the last break, so that the injury will be confined to this; while the conducting power will never be so far impaired as to prevent the passage of the secondary. The dynamic current will, therefore, always have a clean contact, and will never fail to flow.

But while the practical problem immediately in hand receives thus a very easy solution, it is believed that the results arrived at in the experimental inquiry of which the details are given above, may have other applications of sufficient importance to justify their presentation to the attention of the Association.

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## 5. On Magnetizing Locomotive Wheels by Curved Helices, and Experimental Results. By Edward W. Serrell, of Greenfield.

The importance of increasing the power of locomotive engines without adding to their weight, which is so destructive to the superstructure of railways, led me to attempt to magnetize the driving wheels, to obtain additional adhesion.

Before doing so, however, I carefully inquired of those most likely to be informed, both in this country and in Europe, if previously ascertained facts indicated the probability of success, and I was almost discouraged by the unanimous answer—No; but I persevered, and the result is more than was anticipated; an additional adhesion of over seventy-five per cent. has been obtained, and this by a very simple method.

The lower segment of the wheel is surrounded by a helix of copper wire through which the wheel revolves, and, contrary to the generally received opinions, it was found that upon curving the helix into a segment, the radius of which is equal to the diameter of the wheel, the point of greatest magnetic effect coincided with the contact of the wheel and rail.

One wheel had south polarity, and its corresponding opposite wheel north polarity. The wheels magnetized in the experimental trial were $4\frac{1}{2}$ feet in diameter, and weighed about eleven hundred pounds each.

On a very slippery rail 19 + lbs. of steam per inch slipped the wheels without magnetism; under the same conditions 35 + lbs. were required to slip them when magnetized.

On a very clean rail and every thing being favorable, 50 — lbs. were required without any magnetic effect, and 88 + lbs. when magnetized.

The helix was made of No. 8 copper wire in one strand 2,700 feet in length, and laid in 288 turns, insulated with cotton and marine glue and covered with India rubber. I have not been able to discover any increased or diminished effect by the wheels being in motion or at rest, and they were tested up to 300 revolutions per minute.

The battery used was a modification of Grove's, so contrived as not to stop, and consisted of sixteen cups, each having about 300 inches of zinc surface, and they were connected for the quantity of eight cups. I have since adopted a modification of Smee's and Chester's battery, as being more permanent. It should be noticed that when the helices produced the greatest effect they were raised about $2\frac{1}{2}$ inches above the rail, measuring from their under sides. I am indebted to Mr. O. D. Vasmus for aid in the experiments.

## 6. On Nitride of Zirconium. By Prof. J. W. Mallet, of Tuscaloosa, Alabama.

Among the most interesting facts brought to light by the recent researches of Wöhler and Deville upon silicon and the allied elements, is that of the strong affinity of these bodies, when free, for nitrogen. Several of the nitrides which result from this affinity have been described — as those of boron, silicon, titanium, and tantalum. I have now to add to the list *nitride of zirconium.*

This substance was obtained under the following circumstances. The ease with which silicon and boron may be crystallized by exposing the elements in the amorphous state to a very high temperature, in contact with aluminum, which when fused seems to act the part of a solvent, led very obviously to the expectation that other related elements might also be obtained in crystals by this process. Titanium and zirconium suggested themselves as specially worthy of experiment in this direction. With the exception of the bare notice * that Deville, by heating aluminum in a porcelain tube traversed by a mixed current of hydrogen and a vaporized chlorid, had obtained crystalline silicon, boron, carbon, *zirconium*, and *titanium*, I have seen no account of the preparation or properties of the last two. Deville and Wöhler have indeed stated at the beginning of a paper † on nitride of titanium that this substance was first noticed by them in the attempt to procure titanium itself in a compact state — but the means proposed for the attainment of the latter object are not mentioned.

A quantity of amorphous zirconium and titanium was prepared by heating the potassio-fluorids with sodium in an atmosphere of hydrogen, and it then remained to be seen whether the metals could be brought into the crystalline state by exposure to intense heat in contact with aluminum. A small piece of aluminum was placed in the cavity of a lime crucible (of the kind proposed by Deville), and was then surrounded with black pulverulent zirconium, which latter was pressed down as closely as possible, and covered by a layer of quick-lime in powder, also strongly pressed. A stopper of solid lime was fitted to

* Paris correspondence in Amer. Jour. Sci., May, 1856, p. 404.

† Ann. d. Chem. u. Pharm., August, 1857, S. 230.

the opening, and the whole was exposed for about an hour to the heat of a small blast furnace capable of melting platinum.

After cooling, the crucible was removed from the furnace, and was found to be *slightly cracked.* This no doubt occurred at the beginning of the experiment, and was caused by the too rapid application of the blast. On breaking the crucible across, the interior presented the appearance of a porous mass of dark gray color, through which globules of aluminum were scattered. This mass was placed in dilute muriatic acid, and began in part to dissolve with effervescence.

A few iron-black shining scales, like those of graphitic silicon, separated out, and these perhaps constituted the original object of the experiment — that is, were zirconium in the form corresponding to graphite. The color and lustre were very like those of silicon in this form; the scales appeared however to be thin and flat rather than needle-like; no definite angles or planes could be seen under a high microscopic power. In another experiment these scales were obtained in larger proportion, excluding, I think, the likelihood of their being silicon itself, derived either from the aluminum or the lime; the absolute amount, however, was very small, and no chemical examination of the scales could be made.

As the acid continued to act upon the mass taken from the crucible, bright surfaces and little veins of golden color and lustre made their appearance, and here crystalline structure became apparent under a common pocket-lens. It was necessary, however, to use a pretty high microscopic power in order to bring out the form of the very minute specks which formed these gold-like crusts; with a magnifying power of 400–600 they were seen to consist of distinct cubes, the largest of which were not more than the one-hundredth of a millimetre on the side. The color and lustre were those of gold, and the appearance of some of the microscopic specimens was very beautiful, the little cubes being imbedded in a colorless glassy matrix, probably a compound of zirconia and lime. One was reminded by them of the titanium cubes of the iron smelting furnaces.

This gold-colored substance was but very slightly acted on by the common acids, even the nitro-muriatic, or by the alkalies in solution; fused with caustic potash it gave off ammonia in abundance, thus proving the presence of nitrogen. Its composition was not determined quantitatively, owing to want of sufficient material, for much of the zirco-

nium had combined with the oxygen of the air, but a part uniting to the nitrogen. The nitride in contact with water at common temperatures appeared to undergo in some slight degree the same decomposition that Deville and Wöhler * have remarked in the case of nitride of silicon, ammonia being formed.

It having been shown that zirconium is capable of uniting directly with the free nitrogen of the air, one or two experiments were made with gaseous compounds of nitrogen.

Amorphous zirconium was heated in a Bohemian glass tube up to the temperature at which the latter softened, a stream of ammoniacal gas being passed through it. At a low red heat there suddenly appeared a bright glow, spreading rapidly over the metallic powder, and then disappearing; this was probably owing to the presence of a little hydrate of zirconia, the water of which, as Berzelius has shown, yields oxygen to the metal when heated. After cooling, the tube was found to contain a dark gray, perfectly amorphous powder. Under the microscope it could be seen that the gray color was due to a mixture of white and black particles; the white being no doubt zirconia, produced partly by the presence of hydrate as just noticed, and partly by the fact that the ammonia had not been perfectly dried. The gray powder was gently heated in the air to drive off any free ammonia, and then fused with caustic potash; it gave off ammonia in abundance. Heated to low redness in the air, it took fire, glowed brightly, and even continued to burn when removed from the lamp-flame. It burned almost white, and when afterwards fused with caustic potash, gave only traces of ammonia.

A similar amorphous gray powder was obtained by heating the anhydrous chlorid of zirconium in gaseous ammonia, chlorid of ammonium and hydrochloric acid volatilizing. Unfortunately the ammonia was not quite dry, and in consequence the color of the powder was light, showing the presence of but little nitride; on fusion with caustic potash but little ammonia was given off.

Lastly, pulverulent zirconium was heated to bright redness in a tube of Bohemian glass, through which passed a stream of dry cyanogen. The glow alluded to above appeared and spread over the mass. On cooling, an amorphous powder was obtained, of black color with a

* Ann. d. Chem. u. Pharm., Mai, 1859, S. 249.

shade of chocolate-brown; this, after gentle heating in the air, was fused with caustic potash and gave off ammonia in large quantity. Strongly heated in the air, the powder took fire, and burned nearly white; after burning, it gave with caustic potash slight but distinct traces of ammonia. The black powder was not dissolved by muriatic acid, and appeared to be scarcely affected by nitro-muriatic acid. Hot oil of vitriol seemed to act on it but slightly and very slowly; the acid became brown, and a little gas, apparently sulphurous acid, was given off; hence it is probable that this powder contained carbon — was perhaps a nitro-cyanid.

These experiments would seem to show that:

1. Zirconium, like titanium, silicon, and boron, has a strong affinity for nitrogen, is capable of removing it from some of its compounds, and will even unite directly with it when free and inert, as in atmospheric air.

2. The relation, thus indicated, of zirconium to titanium and silicon, supports the evidence afforded by the late experiments of Deville and Troost on the vapor-density of chlorid of zirconium, which appears to have the formula $ZrCl_2$, analogous to $TiCl_2$ and $SiCl_2$.

3. Zirconium has probably not quite as strong an affinity for nitrogen as some of the other elements named above. As prepared from ammonia or cyanogen at least, its nitride burns when strongly heated in the air, like the nitride of niobium of H. Rose,* and perhaps the nitrides of tungsten and molybdenum;† in contact with water nitride of zirconium is probably subject to slow decomposition, like nitride of silicon.‡

4. It would be desirable to examine the action of chlorine upon this nitride of zirconium at a high temperature, so as to ascertain whether cyanogen may exist in any of the specimens prepared by different methods; also to endeavor to obtain the compound in crystals of larger size, and to get a quantitative analysis of it in a state of purity.

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* Ann. d. Chem. u. Pharm., Mai, 1859, S. 140.

† Ibid., Feb. 1858, S. 259.

‡ Ibid., Mai, 1859, S. 249.

## 7. On the Atomic Weight of Lithium. By Prof. J. W. Mallet, of Tuscaloosa, Alabama.

In a paper read before the American Association for the Advancement of Science in August, 1856, I endeavored to show that the equivalent of lithium, which has been usually taken, on the authority of Berzelius, as 6·5 (=81·25 on the oxygen scale), or 6·6 (=82·50), is in fact considerably higher, and may be assumed =7· (or 87·50).

The error involved in the older determinations was noticed as due to the fact, observed by Marignac and others, that when a sulphate (the salt analyzed) is precipitated by an excess of chlorid of barium, traces of the latter are thrown down with the precipitate and cannot be removed by washing, thus bringing out the quantity of sulphuric acid greater, and the atomic weight of the base less than the truth.

My own results were obtained by the method used by Pelouze in determining the equivalents of sodium and barium, namely, the precipitation of chlorid of lithium by a solution of silver of known strength. In this way the equivalent of lithium was found by three experiments =86·93, 86·96, 86·45, or in the mean 86·78 (or 6·95 as referred to the hydrogen unit).

Since the publication of the above result, it has been confirmed by Dumas, who, in one of his recent papers on the equivalents of the elements, states that he has found that of lithium =7·, without however giving the details of the experiments on which this number is based.

On the other hand, Troost, in a paper upon the general history of lithia and its salts,* has objected to the method by which my determination of the equivalent was made, and has returned to a number near that originally given by Berzelius. Troost states that chlorid of lithium on being heated in the air loses chlorine and takes up oxygen, so that it must give by the method of Pelouze an atomic weight for the metal higher than the truth. This fact was distinctly noticed in my former paper, and it was stated that the decomposition might be prevented by addition of a little pure sal-ammoniac to the chlorid of lithium before heating. Troost objects to this, not that he has proved the method of correction defective, but that we cannot in the end tell whether the salt

* Ann. de Chim. et de Phys., [3], t. li. p. 108.

contains its full proportion of chlorine or not, unless the true equivalent of lithium — the constant we are in search of — be known. But it is to be remarked that the product of the exchange of chlorine for oxygen is caustic lithia, exhibiting a strong alkaline reaction. I have twice or thrice prepared chlorid of lithium, adding sal-ammoniac, and igniting in a well-closed platinum crucible, and have always found that several grammes dissolved in a very small quantity of water (the salt is extremely soluble) gave not the slightest alkaline or acid reaction with the most delicate vegetable colors.

Troost himself adopts crystallized carbonate of lithia as the salt to be analyzed in order to determine the equivalent. He precipitates the carbonate, washes it thoroughly, diffuses it in water through which carbonic acid gas is passed until the salt dissolves, evaporates the solution until the carbonate is deposited as a crystalline powder, and dries this powder at 200°. He determines the lithia in one portion of the salt by evaporation with pure sulphuric acid, and the carbonic acid in another portion by noting the loss of weight on fusion with silicic acid. In this way he arrives at the number 6·6 (= 82·5). No proof is offered that exposure to a temperature of 200° is capable of removing every trace of water and all carbonic acid over a single equivalent; yet, unless this be effected, the atomic weight of lithium will be brought out less than the truth. The same result will follow from the mechanical loss of the least drop of fluid during the effervescence of the carbonate with sulphuric acid or the subsequent evaporation of the sulphate of lithia; and, without feeling the slightest doubt of the manipulative skill of the French chemist, we must admit that, in so delicate a process as the determination of an atomic weight, the solution of a carbonate and evaporation of the solution — steps which are generally looked upon as undesirable in the common course of analysis — should, if possible, be avoided.

I have recently made a new determination of the equivalent, deriving it now from experiments upon the sulphate of lithia; applying, however, a method avoiding, as I hope, the source of error to which Marignac has drawn attention; an error which threw much difficulty in the way of his successful estimation of the atomic weights of cerium, lanthanum, and didymium. If we add a salt of baryta in excess to a solution of any sulphate, the precipitate usually contains a small amount of the soluble barytic salt, which cannot be washed out, and which

therefore increases the apparent amount of sulphuric acid present, if the latter be calculated from the weight of the sulphate of baryta, supposed pure. On the other hand, if the soluble sulphate be in excess, it will mix with the precipitate to some extent, and thus the proportion of sulphuric acid may be brought out higher or lower than the truth, as the equivalent of the base under examination is lower or higher than that of baryta. So that, if we wish to determine the atomic weight of lithium, as Berzelius did, by mixing the solution of a known amount of sulphate of lithia with chlorid of barium and weighing the sulphate of baryta precipitated, we are not certain that the weight of the latter really corresponds to the quantity of sulphuric acid in the salt analyzed. The same objection applies to Marignac's analysis of the sulphates of cerium and the allied metals. He there noted the volume of a solution of chlorid of barium of known strength required to precipitate a weighed portion of the sulphate; when a precipitate ceases to form, more or less chlorid of barium may have been used than is really equivalent to the sulphuric acid present.

The amount of the above error must however be constant if the sulphate precipitated, the salt of baryta used, and the circumstances of precipitation be all the same. If the same salt of baryta be used to precipitate *different* sulphates, it is probable that the amount of error will be different for each. But, if we take the sulphates of two very similar and closely related bases, it is probable that *the difference* in the amount of error will be very small. These considerations have led to the following method for determining the equivalent of lithium:

Sulphate of lithia was prepared, with all possible care, from the carbonate, and tested rigidly as to its purity. Two separate portions (A, 1, and 2) of this salt were rendered anhydrous by cautious application of a heat below redness, and accurately weighed. Two similar portions of perfectly pure sulphate of *soda* (B, 1, and 2) were dried and weighed with equal care. And, lastly, two portions of pure sulphate of *magnesia* (C, 1, and 2) were in like manner dried and weighed. Soda and magnesia were chosen for comparison with lithia because the last-named base seems in most of its relations to hold an intermediate place between the two former, with which it is closely allied. Chlorid of barium was also prepared with all the precautions needed to insure its purity, precipitated twice from its aqueous solution by alcohol, and recrystallized three or four times. It was at last obtained as a fine

crystalline powder by stirring the hot saturated solution as it cooled, and this powder was allowed to dry spontaneously in the air at a temperature of about 80° F. Thus prepared, the salt — as Marignac has shown — is not altered in weight by further exposure to air; its theoretical composition is $BaCl + 2HO$; the precise amount of water actually present was probably a little greater, owing to the mode of drying, but was unimportant under the conditions of experiment adopted.

For each of the six weighed portions of sulphates mentioned above, the quantity of chlorid of barium needed for exact precipitation was calculated, assuming the equivalent of sodium = 23, that of magnesium = 12, that of lithium = 7, and that of barium = 68·6, and considering the chlorid of barium as containing strictly two atoms of water. Six portions of the last-named salt were weighed out (at the same time), each less than the amount calculated by one or two centigrammes. Each was dissolved in 200 cubic centimetres of hot water, and added to its corresponding portion of sulphate, likewise dissolved in 200 cub. centim. of hot water. The fluid and precipitate in the six beakers were well stirred, and left to settle.

A solution was now prepared of 1 gramme of the crystallized chlorid of barium (weighed out at the same time with the larger portions) in 1 litre of water, each cubic centimetre corresponding therefore to 1 milligramme of $BaCl + 2HO$. With this standard solution, dropped from a pipette whose degrees = $\frac{1}{5}$th of a cubic centim., the precipitation of the fluid in each of the six beakers was completed — the amount of chlorid of barium thus employed was noted, and added to the weight of the main portion originally taken. At first it was easy to observe the formation of a precipitate on each successive addition of the chlorid of barium solution, and subsidence took place quickly; but, as the point of exact neutralization was more and more nearly approached, each observation became more difficult, and hours and even days were required in order to observe the production of a cloud by each drop added, or to get the fluid clear again for another trial. When the last addition of chlorid of barium altogether failed to produce a precipitate, a single drop of a solution of sulphate of soda was added, and the formation of a cloud noticed.

In this way the following results were obtained:

A, 1.—3·8924 grm. of $LiO, SO_3$ required for complete precipitation 8·6323 grm. of $BaCl + 2HO$ as used.

A, 2. — 4·6440 grm. of LiO, $SO_3$ required 10·2940 grm. of BaCl + 2HO.

B, 1. — 5·0675 grm. of NaO, $SO_3$ required 8·6920 grm. of BaCl + 2HO.

B, 2. — 5·1107 grm. of NaO, $SO_3$ required 8·7688 grm. of BaCl + 2HO.

C, 1. — 4·3380 grm. of MgO, $SO_3$ required 8·8318 grm. of BaCl + 2HO.

C, 2. — 4·6625 grm. of MgO, $SO_3$ required 9·4872 grm. of BaCl + 2HO.

Calculating now from B, and C, the amount of crystalline chlorid of barium necessary to precipitate *an equivalent* of NaO, $SO_3$ or MgO, $SO_3$, we get the following numbers, which represent what may be called *the practical equivalent* of the chlorid of barium as actually used.

| | | Means. | |
|---|---|---|---|
| From B, 1. | 121·78 | 121·80 | 121·96 |
| " " 2. | 121·82 | | |
| " C, 1. | 122·15 | 122.12 | |
| " " 2. | 122·09 | | |

the theoretical equivalent of BaCl + 2HO being 122·1 — the presence of any water over the normal two atoms tends to raise the practical equivalent — the presence of any BaCl in the precipitated BaO, $SO_3$ has the same effect — the presence of either of the soluble sulphates in the same precipitate leads to an opposite result. From this practical equivalent of chlorid of barium and the results given above under A, 1, and A, 2, we may calculate the equivalent of lithium. If we adopt for chlorid of barium the number 121·80 — that obtained by the precipitation of NaO, $SO_3$ — we have for A, 1,

$$\frac{3.8924 \times 121.80}{8.6323} = 54.92 = \text{LiO, } SO_3$$

$$54{\cdot}92 - 48 \text{ (i. e. } SO_3 \times O) = 6{\cdot}92 = \text{Li,}$$

and for A, 2,

$$\frac{4{\cdot}6440 \times 121{\cdot}80}{10{\cdot}2940} = 54{\cdot}95 = \text{LiO, } SO_3$$

$$54{\cdot}95 - 48 = 6{\cdot}95 = \text{Li.}$$

The mean of the two results is 6·935.

If we take for chlorid of barium the number 122·12 — derived from the experiments with MgO, $SO_3$ — we get by a similar calculation,

From A, 1, . . . . . Li = 7·07
" " 2, . . . . . Li = 7·09

or, in the mean, 7·08.

Lastly, if we take the mean of the two numbers for chlorid of barium, namely, 121·96, we get for

A, 1, . . . . . Li = 6·99
" 2, . . . . . Li = 7·02

or, in the mean, 7·005.

Hence we find that the equivalent of lithium, as deduced from the mean results of the above experiments, comes out

6·935 (= 86·69 on the oxygen scale)
7·080 (= 88·49 " " " "
or 7·005 (= 87·56 " " " "

as we take the *practical equivalent*, or actual precipitating power, of chlorid of barium from the experiments with NaO, $SO_3$, those with MgO, $SO_3$, or the mean of the two; these numbers exhibiting close agreement, and obviously indicating 7· as the true equivalent of the metal. It will be observed that the above method is independent of a knowledge of the exact equivalent of barium, and uses chlorid of barium merely as a means of bringing sulphate of lithia into comparison with the sulphates of soda and magnesia — the equivalents of the two last-named bases may be considered as ranking among those best established — and the small difference between *the practical equivalents* for chlorid of barium deduced from these two shows the probable extent of error involved in the assumption of the same constant in the precipitation of the sulphate of lithia.

While these results confirm those formerly obtained by the analysis of chlorid of lithium, I do not consider them of superior, or perhaps even of equal value. The estimation of chlorine by the method of Pelouze is apparently one of the most simple and exact processes for the determination of an atomic weight which have ever been proposed, and it is, as I believe, fully applicable to the case of chlorid of lithium.

As the result of both sets of experiments we may fairly take the number 7· (= 87·50) as representing the true equivalent of the metal.

## 8. Contributions to the History of Gypsums and Magnesian Rocks.* By T. Sterry Hunt, of Montreal, Canada.

The gypsums found in nature may be divided into two classes, those directly deposited from water, and those produced by the alteration of beds of limestone. To the latter division belong the gypsums found in the vicinity of solfataras, where, as Dumas has shown, the slow oxydation of moist sulphuretted hydrogen gives rise to sulphuric acid, which transforms beds of carbonate into hydrated sulphate of lime. We must equally refer to the same class those gypsums which are formed among calcareous rocks by the action of waters containing free sulphuric acid. Such a process I have long since described in Western Canada, where numerous springs containing besides sulphates of lime, magnesia, oxyd of iron, alumina, and sulphuretted hydrogen, three or four thousandths of free sulphuric acid, rise through Upper Silurian strata, in the calcareous portions of which they sometimes give rise to masses of gypsum.

Bischof (*Chem. Geology*, i. 418), who does not appear to have seen my analyses of these acid waters, rejects my view of the epigenic origin of these masses of gypsum, although it will be apparent to every one who examines the facts, that the action of such waters upon calcareous strata must give rise to sulphate of lime. I do not, however, confound these recently formed masses of sulphate of lime with the older gypsums, which, associated with dolomites, sea-salt, and sulphur, are abundant in the Saliferous or Onondaga salt group of the same region. — *Am. Jour. Sci.* [2], vii. 175; *Report Geol. Survey*, 1848, 150; *Comptes Rendus de l' Acad.*, 1855, xl. 2348.

These acid waters, which make their appearance in an almost undisturbed region, I conceive to have their origin in deeply buried strata, where gypsum or other sulphates may be undergoing decomposition by the action of water and silica at an elevated temperature, a process analogous to that which gives rise to exhalations of carbonic acid gas.

Waters containing free sulphuric acid or ferric or aluminous sulphate, may, by flowing into basins where carbonate of lime is present, give

* See page 247.

rise to solutions of sulphate of lime, and the evaporation of these, of sea-water, or other gypseous solutions, must give rise to deposits of sulphate of lime, which will belong to the first division mentioned above. These modes of formation however do not account for an important fact in the history of most stratified gypsums, which is that of their almost constant association with carbonate of magnesia, generally in the form of magnesian limestone. Beds of dolomite are often interstratified with, or include, beds or masses of gypsum, while dolomite and carbonate of magnesia are sometimes found imbedded in gypsum or anhydrite. For a description of the magnesite which is disseminated in the gypsum of Salzburg, see Dufrénoy, *Minéralogie*, 2d ed., ii. 424. Small masses of compact and crystalline gypsum, occasionally associated with crystals of calcite and quartz, abound in some of the dolomite beds of the so-called Calciferous sandrock in Canada, and crystallized gypsum and anhydrite, together with sulphates of baryta and strontia, and fluor spar, occur in geodes in the magnesian limestone of Niagara. The anhydrous sulphate of lime not only forms beds by itself, but is often met with disseminated in masses, grains, or crystals through beds of gypsum, and even interstratified with it, as in the south of France, in the Hartz, Switzerland, and in Nova Scotia, as described by Mr. Dawson. — (*Acadian Geology*, 225.) The conversion of beds of anhydrite into gypsum by the absorption of water, and the attendant phenomena, have been described by Charpentier.

Both the hydrous and anhydrous sulphates sometimes form the cement of conglomerates or breccias, which enclose flints, fragments of shale and of limestone, as at Pomarance in Tuscany (Scarabelli, *Bull. Soc. Géol. de France* [2], xi. 346), and also at Bex, where the cement of the conglomerate is a granular anhydrite (Charpentier, *Ibid.* [2], xii. 546).

Gypsums, moreover, often include clay and sand, and sometimes contain a considerable admixture of carbonate of lime, which in those of Aix, according to Coquand, amounts to eight per cent. The gypsums of Montmartre also contain, according to Delesse, besides some clay and sand, and several hundredths of carbonate of lime, not less than three per cent. of soluble silica intermixed. Silica in the form of flint or chert is sometimes found in concretions with gypsum; thus in the miocene clays near Bologna in Italy, flints are met with associated with

sulphates of lime, of baryta and strontia, together with pyrites and sulphur. Masses of sulphate of strontia are likewise found in clays with the gypsums of Montmartre, and the association of sulphate of strontia with the sulphur, gypsum and rock salt of Sicily is well known. The gypsums of Madrid, which occur in tertiary clays, are, according to Casiano de Prado, accompanied by beds of chert and of magnesite.—*Bull. Soc. Géol. de France* [2], xi. 334.

Besides the rock salt which so often occurs with gypsum, we may here recall its frequent association with the sulphates of soda and magnesia, both of which are found in very many places imbedded in gypsum, or intermingled with rock salt or with the associated clays. (Bischof, *Chem. Geology*, ii. 421–431.) Large deposits of both of these sulphates occur with gypsum and rock salt in Spain; in Nova Scotia also sulphate of soda is found in gypsum with boro-calcite, an association worthy of notice from the occurrence of boracite, both crystallized and massive (stassfurthite) with gypsums in Germany.—(How, *Am. Jour. of Science* [2]. xxiv. 230.)

The gypsums of the class which we are now describing appear in every geological period. To these apparently belong the masses of gypsum and anhydrite, which at Fahlun are associated with dolomite and serpentine in the chloritic bands of the oldest crystalline rocks of Scandinavia, the probable equivalents of the Laurentian system of North America. On this continent the oldest known gypsums are those already mentioned as occurring near the base of the palæozoic series, and in what is called by the geologists of New York the Calciferous sand-rock. As we ascend the series gypsum is occasionally met with in the Clinton and Niagara groups, until we reach the Onondaga salt-group in the Upper Silurian rocks of Canada and New York, which contains great deposits of dolomite and gypsum, occasionally accompanied by sulphur. The gypsums, anhydrites, and brine springs of Nova Scotia belong to the Carboniferous series, while the frequent recurrence of gypsum in Europe through all the higher rocks up to the Miocene inclusive, is to well known to require notice.

The so-called primitive gypsums and anhydrites, which in the Alps and Pyrennees occur interstratified with crystalline schists, are now known to belong to altered secondary strata. These gypsums enclose many crystalline minerals, such as talc, mica, epidote, hornblende, dipyre, beryl, quartz, hematite, blende, and pyrites. At Saurat in the

Pyrennees many of these minerals appear in the vicinity of a mass of granite which penetrates and alters both fossiliferous limestone and gypsum. The latter becomes mingled with and finally passes into limestone. (Coquand, *Bull. Soc. Géol. de France* [1], xii. 345.) In Algiers, where gypsum is associated with crystalline limestone, gneiss, amphibolite, and serpentine, small crystals of beryl are found disseminated alike through the limestone and the gypsum. Some of the gypsums of the Hartz, according to Frapolli, contain nodules of a silicate of magnesia, colored by carbonaceous matter, and having the softness and the chemical composition of steatite. — (*Ibid.* [2], iv. 832.)

The marine origin of the greater number of gypsiferous formations is evident both from the accompanying rock salt and the associated fossils, but certain gypsums (as well as certain dolomites) have evidently been deposited in fresh-water basins. A gypsum from Asia Minor examined by Ehrenberg contains a great number of fresh-water polygastric infusoria, and beds of gypsum occur in the lacustrine basins of Aix and of Auvergne; the gypsiferous strata of the Paris basin are also regarded as of fresh-water origin.

Besides the magnesian limestones of gypsiferous strata great deposits of dolomite occur in the rocks of every geological period. I have long since described the dolomites which form extensive beds, often associated with ophiolites and with crystalline limestones, in the Laurentian system in Canada. Great portions of the palæozoic limestones of North America are magnesian, especially in the valley of the Mississippi,*

* For the following facts with regard to the dolomites of the palæozoic rocks of the Mississippi valley, I am indebted to Prof. James Hall of Albany. We have there in ascending order:

1st. The so-called *Lower Magnesian limestone*, which is regarded as the equivalent of the Calciferous sandrock, and is from 200 to 250 feet in thickness. It is the lead-bearing rock of Missouri, and probably contains the cobalt ores of that region.

2d. The *Galena limestone*, consisting of about 250 feet of dolomite interposed between the Trenton and the Hudson River groups. It is the lead-bearing rock of Iowa, Wisconsin, and Illinois.

3d. The *Niagara limestone*, also dolomitic, about 250 feet in thickness, and sometimes holding galena and blende.

4th. The *Leclaire* or *Galt limestone*, a dolomitic formation interposed between the

while deposits of dolomites are found in Europe alike in the Permian, Triassic, Jurassic, and Tertiary strata. Mr. Dana has even described as of recent formation a dolomite from the coral island of Matea, examined by Silliman and myself.—(*Am. Journal of Science* [2], xix. 429.)

The mechanical conditions of these magnesian limestones vary greatly; they are sometimes made up of crystalline grains of dolomite, which are strongly coherent, or more rarely form a loose sand. Not unfrequently the magnesian limestones are concretionary in their structure, and may be oolitic or botryoidal. The action of the concreting force has sometimes obliterated the marks of stratification. The porous or cavernous structure of many dolomites is also to be remarked.

Magnesian limestones often contain large admixtures of clay and sand; dolomite is not unfrequently the cement of breccias or conglomerates, as in the well-known conglomerate of the Permian system in England. Concretionary masses of dolomite sometimes occur in these aggregates, and in the Permian rocks of the Vosges are found in beds of a sandy clay, itself occasionally cemented by dolomite.

I have elsewhere described two remarkable dolomitic conglomerates from the palæozoic rocks of Canada. The first of these belongs to the upper portion of the Hudson River group, and is conspicuously seen at Pointe Levis and on the island of Orleans. The associated rocks are there graptolitic shales, sandstones, and fossiliferous limestones, together with great masses of a greenish or grayish-white subtranslucent compact concretionary limestone. This is without distinct marks of stratification, exhibits no trace of organic remains under the microscope, and has all the characters of a travertine or calcareous sinter. Interstratified with this last are beds of bituminous yellow-

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last and the Onondago salt group. It attains upon the Mississippi a thickness of 500 feet, but thins out to the eastward.

5th. The magnesian limestones of the *Onondaga salt group*, 100 feet thick.

6th. A dolomitic deposit in the upper part of the Carboniferous series.

[The Calciferous sandrock throughout Lower Canada, includes extensive beds of dolomite, and the Hudson River group is characterized by beds of dolomite and of magnesite. The oldest known Silurian dolomites on Lake Huron in Canada are not older than the Birdseye or Chazy, and are immediately underlaid by a sandstone which resembles that of St. Mary's on Lake Superior.] T. S. H.

weathering dolomite, containing carbonate of iron, and always intermixed with more or less sand or clay or both; the clay in one specimen amounted to fifty per cent., while another quartzose variety gave carbonate of lime 53·04, carbonate of magnesia 31·96, carbonate of iron 5·80, silicious sand 8·80 = 99·60. The latter is a friable crystalline rock, showing in its fracture broad surfaces of cleavage, like the crystals of Fontainebleau sandstone. These dolomites, which contain no fossils, are occasionally traversed by veins of quartz and calcareous spar, or contain small masses of the latter mineral, apparently filling cavities. They are interstratified alike with the travertines and with the fossiliferous limestones, sometimes in large beds, and at other times in lenticular masses, or in layers of a few lines in thickness separating masses of the travertine.

The conglomerates of this series enclose in a paste of ferriferous dolomite, grains and rounded fragments of limestone, often having the characters of the associated travertine, together with fragments of quartz and argillite, and small masses of a nearly pure yellowish crystalline dolomite; these are perhaps concretionary in their origin, and not imbedded fragments. Other beds of a similar conglomerate occur in the same series having a cement of pure carbonate of lime, and the travertine itself often encloses grains of sand. — (*Geol. Surv. Canada; Report*, 1853–56, p. 465.)

The other conglomerate to be noticed occurs on the islands of Montreal, St. Helens, and several other localities in the neighborhood, and belongs to small detached patches of the Lower Helderberg series, left after denudation, which repose unconformably alike on Lower Silurian and Laurentian rocks. In some localities they enclose the peculiar feldspars of the latter, in others the fossiliferous limestones, shales, sandstones, and cherts of the former series, while in others still the principal elements are black augite, mica, and olivine, derived from the igneous rocks which in this vicinity have broken through the Lower Silurian series. These conglomerates, which are remarkable for their great coherence, have a greenish, bluish, or grayish yellow-weathering base, and contain much carbonate of iron. The soluble portion of a specimen from St. Helens was equal to 46·0 per cent., and consisted of carbonate of lime 57·8, carbonate of magnesia 16·4, carbonate of iron 25·8 = 100·0. In one instance these yellow-weathering beds of conglomerate are associated with others of which the cement remains white

on the exposed surfaces, effervesces freely with acids, and is pure carbonate of lime. — (*Ibid.*, 1857, 201.)

Dolomite also occurs filling up fissures and cavities in other rocks, as in the case of pearl-spar in geodes and veins. The black and yellow marble from northern Italy, known under the name of Portor, and belonging according to Savi, to the Neocomian formation, is composed, by my analysis, of a black, nearly pure limestone containing only one hundredth of carbonate of magnesia, penetrated by veins of ferriferous dolomite, which gave me 35·5 p. c. of carbonate of magnesia, and 4·6 of insoluble silicious matter, the residue being carbonate of lime and a little carbonate of iron. The veins of magnesian carbonate sometimes give to the Portor the aspect of a breccia.

Examples of the apparent infiltration of dolomite occur in black bituminous limestones at Montreal and Ottawa, belonging both to the Trenton and Chazy divisions. These limestones, which contain only traces of magnesia, enclose casts of the interior of *Orthoceras Murchisonia*, and *Pleurotomaria*, consisting of a gray crystalline dolomite, weathering reddish, and appearing in high relief upon exposed surfaces of the limestone. In both localities the limestones are traversed by thin, irregular veins of a similar dolomite, which communicate with the casts. By the action of dilute hydrochloric acid the limestone matrix is dissolved, and it is seen that the cavity of the fossil is in many cases only partially occupied by dolomite; that portion which is uppermost in the stratum being often filled with carbonate of lime to the extent of one third or one fourth, but in other specimens the whole cast is of dolomite. In some of the larger casts there are drusy cavities lined with crystallized dolomite and occasionally containing prisms of quartz. The analysis of a fragment of the cast of an *Orthoceras* from the Trenton limestone at Ottawa, gave me carbonate of lime 56·00, carbonate of magnesia 37·80, carbonate of iron 5·95 = 99·75. The surrounding limestone, which was compact, bluish-gray, and bituminous, contained 3·9 p. c. of clay and sand; its solution gave 0·6 p. c. of oxyd of iron with alumina, but no magnesia. Similar examples of fossils replaced by dolomite occur in gray limestones associated with the travertines and dolomites of Pointe Levis.

Magnesian limestones are very frequently destitute of organic remains; in some cases, however, they may contain calcareous fossils, as in the Niagara limestone at Dudswell, where corals of the genera *Cya-*

*thophyllum*, *Porites*, and *Favosites*, composed of pure carbonate of lime, and generally bluish-black in color, are imbedded in a yellow ferriferous magnesian limestone which contains an excess of carbonate of lime. This limestone gave by analysis carbonate of lime 56·60, carbonate of magnesia 11·76, carbonate of iron 3·23, insoluble quartz sand 26·72 = 98·31. The portion soluble in cold dilute acetic acid was carbonate of lime with four per cent. of carbonate of magnesia and a trace of iron, and the residue when digested with dilute hydrochloric acid left 52·0 p. c. of sand and pyrites; the dissolved part consisting of carbonate of lime 51·75, carbonate of magnesia 35·73, carbonate of iron 12·52 = 100·00.

In the magnesian limestone of Galt in western Canada, which is a pure crystalline dolomite, there are numerous casts of bivalve molluscs, the shells of which were evidently removed by solution after they had been filled and enveloped by the dolomitic matrix, since the walls of the cavities once occupied by the shells of a large bivalve, *Megalomus Canadensis*, retain the markings of the inner and outer surfaces of the shell. Similar moulds of *Ophileta compacta* are abundant in the blue dolomite of Beauharnois, which belongs to the Calciferous sandrock; in a dolomite of the same geological formation from the Mingan islands, the shells of *Ophileta*, *Maclurea*, and *Scaphites* are replaced by silica.

In some portions of the Galt formation, fragments of encrinal columns are found replaced by dolomite, which is only distinguished by a little difference of color from the matrix. It would appear in this case as if the calcareous fossil, having been first removed by solution, the cavity had been subsequently filled with dolomite as in the casts found in the Ottawa and Montreal limestones.

Although dolomites not unfrequently form by themselves masses of great thickness, as in the Jurassic formation of the Tyrol and the palæozoic rocks of the west, they are often interstratified in an intimate manner with pure limestones. Such is the case with the ferriferous dolomites already noticed in describing the dolomitic conglomerates and travertines of Pointe Levis. In the Chazy limestone of Montreal, thin, irregular layers of reddish ferriferous dolomite, themselves filled with encrinal columns, are interposed between beds of fossiliferous limestone. The magnesian layers being pulverulent, the encrinal columns, which are pure carbonate of lime, are easily separated from

their matrix, which gave me carbonate of lime 40·95, carbonate of magnesia 24·19, carbonate of iron 27·03, silicious sand without alumina, 9·01 = 101·18; the iron was in part as peroxyd. The bluish crystalline limestone distant an inch from the magnesian layer gave 18·4 p. c. of white insoluble residue and 1·09 p. c. of carbonate of magnesia.

In these strata we sometimes meet with similar reddish pulverulent layers which contain no carbonate of magnesia, but are composed of carbonate of lime with a large amount of peroxyd of iron; such a mixture in one instance forms the cement of a breccia of fragments of the blue limestone; it was perhaps at one time a double carbonate of lime and iron.

The thin beds of dolomite above described are closely associated with those holding the dolomitic casts of orthoceratites already noticed; these were enclosed in a nearly black compact limestone, which, during its solution in hydrochloric acid, evolved traces of sulphuretted hydrogen. The residue contained a little iron pyrites, which was removed by nitric acid; it was black from carbonaceous matter, but became white by ignition in the air, and was an impalpable powder, equal to 12·8 p. c. of the rock. Dilute soda ley removed from it 9·5 p. c. of its weight of soluble silica, and the residue had nearly the composition of a feldspar. It gave me, silica 73·02, alumina 18·31, lime 0·93, magnesia 0·87, potash 5·55, soda 0·89 = 99·57.

The fossiliferous yellow magnesian limestones of Dudswell are in like manner interstratified with beds of gray crystalline limestone containing 6·3 p. c. of sand and only 1·3 p. c. of carbonate of magnesia. These beds having been much disturbed and broken, the interstices appear to have been filled up with portions of the yellow magnesian paste, giving rise to a marble which in some portions resembles the so-called Portor.

We see from the above examples that dolomites may occur interstratified both with limestones of organic origin and with others which are evidently chemical deposits. Allied to these latter are certain porous tufaceous beds of carbonate of lime which sometimes accompany dolomite. Such tufas occur alternating with the dolomites and gypsiferous marls of the Onondaga salt group. A similar layer of cellular calcareous tufa, free from magnesia, I have observed immediately covering a deposit of crystalline incoherent dolomite in the Eocene series at

Pont St. Maxence in France. (See also, Damour, *Bull. Soc. Géol. de France* [2], xiii. 67.)

The chemical constitution of the rocks containing carbonate of magnesia now demands our consideration. Pure dolomite is well known to consist of equivalents of carbonate of lime and magnesia corresponding to 45·65 parts of the one to 54·35 of the other, and many magnesian limestones have this composition, or contain beside only mechanical impurities, such as sand and clay. Others, with an excess of carbonate of lime, are shown by the method of Karsten to be mixtures of dolomite with carbonate of lime, which is readily separated by the solvent action of cold dilute acetic acid. The same chemist, however, found in clefts and fissures of the gypsiferous rocks of Luneberg and elsewhere, carbonates of lime and magnesia mingled with clay, from which dilute acetic or muriatic acid removed the whole of the lime, leaving a residue of from 4·0 to 68·0 p. c. of magnesian carbonate, which had evidently been mechanically intermingled with the carbonate of lime.—(Bischof, *Lehrbuch*, ii. 1161.) Since the presence of sulphate of lime appears to prevent, in a great measure, the union of the two carbonates, we might suppose that the association of gypsum with these magnesian clays had in some way hindered the formation of the double carbonate. The free carbonate of lime which they contain is, however, probably epigenic and produced by the decomposition of a portion of the magnesite by the infiltration of dissolved gypsum.

Carbonate of iron often replaces a part of the magnesian carbonate in dolomites, which also sometimes contain carbonate of manganese, and even carbonates of zinc, cobalt, and lead. It not unfrequently happens that the sum of the other carbonates in these ferruginous dolomites is more than equivalent to the carbonate of lime. Such is the case with the dolomite from the Chazy formation at Montreal.

The dolomites of the Hudson River group in eastern Canada are very often associated with copper, nickel, titanium, chrome, and manganese. A grayish granular dolomite from Sutton, which contains disseminated chlorite and crystals of magnetite, weathers blackish-brown from the presence of manganese. The foreign minerals are arranged in bands, and layers of the dolomite an inch or two in thickness are apparently free from admixture. The analysis of such a portion gave me, carbonate of lime 40·10, carbonate of magnesia 20·20, carbonate of iron 10·65, carbonate of manganese 7·65, insoluble, chiefly quartz,

21·45 = 100·00. The associated crystals of magnetite contained no trace of manganese.*

Magnesian limestones containing an excess of carbonate of magnesia are not uncommon; one from the muschelkalk of Thuringia gave to Senft, carbonate of lime 42·9, carbonate of magnesia 55·4, besides 2·7 of carbonate of iron = 101·0. A lacustrine dolomite from the brown-coal formation near Giessen contains, according to Knapp, carbonate of lime 42·80, carbonate of magnesia 49·63, besides oxyd of iron and impurities, and a specimen from the Lower Magnesian limestone from Lake Superior, gave to Whitney, carbonate of lime 25·28, and carbonate of magnesia 32·57, besides 37·0 of sand and a little iron and alumina.

Similar magnesian rocks are described by Alberti as occurring in the variegated marls of the *keuper* or upper part of the Triassic system in Germany. A tender greenish schistose marl from Tubingen effervesced very slightly with acids, and gave for 100·00 parts, carbonate of lime 14·56, carbonate of magnesia 19·10, the remainder being clay with a little iron-oxyd. — (Senft, *Die Felsarten*, 134.) Von Bibra has described similar magnesian marls from the muschelkalk in Franconia (Bischof, *Lehrbuch*, ii. 1158), and Gueymard from the gypsums of Ro-

* Carbonate of manganese is frequently met with in the rocks of this geological series, causing them to weather brownish-black. I have described an impure chloritic limestone of this kind from Granby (Canada East), which contains besides protoxyds of manganese and iron, portions of chrome, nickel and titanium. — Report, 1853–56, 474, and Amer. Jour. Sci. [2], xxvi. 238. Rogers has in like manner noticed the occurrence of a large proportion of protoxyd of manganese in the olive-colored slates of the Lower Silurian series in Pennsylvania, and to the decomposition of such rocks correctly ascribes the origin of the deposits of peroxyd of manganese met with in that region. Beds of silicate of manganese, more or less intermingled with carbonates of manganese and lime, are interstratified with crystalline schists in various localities in New England. I may mention in this connection a compact massive carbonate of manganese which is said to occur in slates supposed to be of Silurian age, at Placentia Bay, Newfoundland, and which I received from Dr. J. W. Dawson. It is conchoidal in fracture, translucent on the edges, with a feeble waxy lustre; color, fawn to pale chestnut-brown. H. 4·0. D. 3·25. It is penetrated and incrusted in part with crystalline peroxyd of manganese. Acids in the cold scarcely attack this mineral, but heated nitric acid dissolves it with effervescence, leaving a residue of 14·4 p. c. of silica, of which the greater part is soluble in a dilute alkaline solution. The analysis gave me besides 84·6 p. c. of carbonate of manganese, and traces of lime, iron and magnesia. — Report, 1857, 204.

quevaire in Provence. The bituminous salt-clays (*salzthon*) which occur with gypsum and rock salt, when freed by washing from soluble salts, contain, according to Schafhautl, carbonates of magnesia and iron, often with very little carbonate of lime, the argillaceous matter varying from 12·0 to 70·0 p. c. — (Bischof, *Lehrbuch*, ii. 1725.) To these clays are perhaps related the magnesian marls examined by Karsten (§ 56). Völckel has described a dark gray rock interstratified with limestone from the *keuper* near Solothurn, and consisting of carbonate of magnesia 54·55, carbonate of iron 33·94, carbonate of lime 0·67, with 10·81 of clay, water, etc. — (*L. and K. Jahresbericht*, 1849, 581.)

Magnesian rocks allied to the last occur in the Hudson River group of eastern Canada, and were described by me several years since. In the township of Sutton, interstratified with dolomite, steatite, and talco-quartzose strata, is a bed of green and white reddish-weathering crystalline rock, gneissoid in structure, and containing variable proportions of magnesian carbonate. A pure and nearly white fragment gave to hydrochloric acid, carbonate of magnesia 83·35, carbonate of iron 9·02, and left insoluble 8·03 = 100·40; while another specimen from the same mass contained, carbonate of magnesia 33·00, carbonate of iron 19·35, alumina 0·50, insoluble 45·90 = 98·70. In both cases the solution contained a little nickel, which occurs in the rock, in part at least, in the form of grains of nickeliferous pyrites. The insoluble portion is a silicate of alumina and alkalies, with a little magnesia, and appears to consist of a mixture of feldspar with a little mica and talc, the latter minerals being colored emerald-green by a small portion of oxyd of chrome.

In the township of Bolton there occurs a bed of magnesite many yards in breadth, interstratified between steatite on the one side, and an impure ophiolite passing into diorite, on the other. It is made up of brilliant cleavable grains of magnesian spar, bluish-gray or nearly white in color, and intermingled with others of white hyaline quartz, which sometimes forms small irregular veins. One of several analyses of this rock gave me, carbonate of magnesia 59·13, carbonate of iron 8·32, insoluble 32·20 = 99·65. In other specimens the proportion of carbonate of iron is a little greater, and traces of carbonate of lime are sometimes met with, while nickel is never wanting and sometimes coats the joints of the rock with a yellowish-green film of what appears to be

a hydrocarbonate of nickel; the proportion of this metal determined upon a considerable quantity of the rock was found equal to about one thousandth. The insoluble residue from this magnesite was greenish-gray in color, and gave by analysis 93·6 p. c. of silica, besides some alumina, 0·8 of alkalies, and traces of lime, magnesia, and oxyd of chrome, which gives an emerald-green color to some portions of the rock. I have already shown that nickel is rarely absent from the magnesian rocks of this region, where it is generally accompanied by chrome. These magnesites in powder do not perceptibly effervesce with cold hydrochloric acid, which however readily dissolves them with the aid of heat. The decomposition of the contained carbonate of iron renders their weathered surfaces reddish-brown and pulverulent. — (*Report*, 1853–56, p. 460.)

I have detected a quartzose magnesite closely resembling that of Bolton, containing nickel, and stained emerald-green by oxyd of chrome, among a collection of rocks brought from California by Mr. W. P. Blake, who also found a bed of nearly pure white compact carbonate of magnesia among the crystalline schists of that region. I may here recall the existence of beds of magnesite among argillites in Styria, and also in the ancient crystalline gneiss of Modum in Norway, where a crystalline magnesite is the gangue of crystals of serpentine and ilmenite. — (*Am. Jour. of Science.* [2], v. 389.)

The greater number of dolomites and magnesian rocks are shown by their fossils or by the nature of the associated strata to be of marine origin, but dolomites are also found in fresh-water deposits. Such is that, with excess of magnesian carbonate from the brown-coal formation near Giessen (§ 57), and dolomites are said to occur with the lacustrine limestones of Dachingen near Ulm. — (Senft, *Die Felsarten*, 133.)

Having in the preceding pages brought together the principal facts in the history of magnesian rocks, as well from the researches of others as from our own observations, we have seen that these rocks consist essentially of dolomite, mixed with carbonate of lime on the one hand, and with carbonates of magnesia and iron on the other, passing thus into magnesite. The frequent intermixtures of sand and clay and even of fragments of quartzite, argillite, and limestone, clearly show their sedimentary origin, which is moreover rendered evident by the fact that they are often interstratified with pure limestones and even inclose calcareous corals; these facts exclude the idea of the formation

of all such dolomites at least, by the alteration of deposits of carbonate of lime as supposed by Von Buch, Haidinger, and Favre.

The dolomites of the Tyrol which Von Buch imagined to have been formed from the alteration of limestones by magnesian vapors evolved at the time of the ejection of certain melaphyres of that region, have been shown to be much more recent than these melaphyres, which according to Fournet are not intrusive but sedimentary rocks, probably of Carboniferous age, altered *in situ*. These metamorphosed strata are separated from the dolomites, which are Jurassic, by unaltered Triassic strata, including the muschelkalk and a conglomerate holding rolled fragments of the melaphyres.* (*Bull. Soc. Géol. de France* [2], vi. 506–516.) In several other cases where dolomization was supposed to have been produced by the proximity of igneous rocks, Delesse and Delanouë have shown that the change had been limited to an alteration in the texture, and that there had been no addition of magnesia.

Favre supposes with Haidinger that magnesian solutions under heat and pressure have given rise to dolomites by decomposing beds of limestone with formation of carbonate of magnesia agreeably to the observations of Von Morlot and Marignac. (*Ibid.* [2], vi. 318.) This hypothesis is evidently not applicable to those magnesian limestones which include beds, fragments, or organic remains of pure carbonate of lime. In any case we must suppose a long continued filtration of solutions of magnesian chlorid through the heated limestone under certain conditions, which seem at least improbable.

The theory of the formation of magnesian sediments will be readily understood from the experiments which have been described in the earlier parts of this paper,† but before proceeding to its consideration I wish to call attention to the results of the concentration by evaporation of natural waters in basins without an outlet. If such a basin contain sea-water, the gypsum, being insoluble in a saturated brine, will be entirely deposited before the crystallization of the sea-salt, and there will remain a liquid containing no lime-salts, but chlorids of sodium and magnesium with a large amount of sulphate of magnesia. Such are

* Bischof cites *Fournet, Histoire de la Dolomie*, 1847, but I have not been able to consult the work in the preparation of this paper.

† See note to page 247.

the waters of Lake Elton and many of the brine pools of the Russian steppes, while on the contrary the saturated brines of the Dead Sea and some other salt lakes contain little sulphate but abundance of chlorid of calcium, and if they are the residues of sea-water, have been modified by additions of this salt, which has converted the sulphate of magnesia into chlorid of magnesium and gypsum, the calcareous chlorid remaining in excess.

But while some of these saline lakes may be supposed to be basins of sea-water, modified by evaporation either alone or conjoined with the influx of foreign saline matters, others were evidently once fresh-water lakes in which, the loss of water being equal to the supply, have gradually accumulated the soluble salts of all the rivers and springs flowing into the lake. We may arrive at some notion of the diverse natures of the different saline lakes which would be formed in this way if we suppose the waters of different European rivers to be subjected to evaporation under conditions like those of the salt lakes of Western Asia. In the waters of the Elbe and Thames chlorids greatly predominate (in the latter with gypsum), with small amounts of magnesian salts, and the evaporation of these waters would give rise to lakes containing a large proportion of common salt. In the Seine, on the contrary, sulphate of lime predominates, while the waters of the Rhine, the Danube, the Arr, and the Arve contain but small amounts of chlorids and large proportions of sulphates of lime and magnesia.

In other rivers we find alkaline salts; the Loire at Orleans, according to Deville, contains in 100,000 parts, 13·46 of solid matters, of which 35·0 p. c. is carbonate of lime, 30·0 p. c. silica, while two thirds of the more soluble salts consist of carbonate of soda. In the waters of the Garonne, with as large a proportion of silica, and more carbonate of lime, the carbonate of soda equals one fourth of the soluble salts, while 100,000 parts of the water of the Ottawa, according to my analysis, contain 6·11 parts of solid matters, consisting of carbonate of lime 2·48, carbonate of magnesia 0·69, silica 2·06, sulphates and chlorids of potassium and sodium 0·47, and carbonate of soda 0·41. (*Report Geol. Survey of Canada*, 1853–56, 360, and *Philos. Mag.* [4], xiii. 239.) Silica, although more abundant in alkaline river waters which are chiefly derived from crystalline rocks, is not wanting in waters containing neutral earthly salts, like the Seine and the Rhone, of the solid

matters of which, according to Deville, it forms respectively 10·0 and 13·0 p. c. — (*Ann. de Chem. et Phys.* [3], xxiii. 32).

The waters which rise from the Lower Silurian shales of the St. Lawrence valley are, as I have elsewhere shown, remarkable for the predominance of alkaline salts, which sometimes amount to one thousandth, or more than one half the solid matters present; these waters are distinguished from the river waters just mentioned by their comparatively small amount of silica and earthy carbonates, and by the presence of a notable proportion of borates. — (*Rep. Geol. Survey of Canada*, 1852, p. 165, — 1853–56, p. 469, and *Proc. Royal Soc., Phil. Mag.* [4], xvi. 376.)

We may here refer to the strongly alkaline waters furnished by the artesian wells of Paris and London as evidences of the abundance of alkaline carbonates in natural waters, and to the springs of Vichy and Carlsbad, the latter of which, according to the calculations of Gilbert, furnish annually more than thirteen millions of pounds of carbonate of soda. The evaporation of these alkaline waters, whether rivers or springs, must give rise to natron lakes like Lake Van and those of the plains of Araxes, Lower Egypt, and Hungary. — (Bischof, *Lehrbuch*, ii. 1143).

The carbonate of soda contained in these waters has its source in the decomposition of feldspathic minerals, and shows the continuance in our time of a process whose great activity in former geologic ages is attested, as I have elsewhere maintained, by vast accumulations of argillaceous sediments deprived of a large portion of their soda, and also by the carbonate of lime which by the intervention of carbonate of soda has been formed from the chlorid of calcium of the primeval ocean and deposited as limestone.

An indispensable condition for the precipitation of carbonate of magnesia is the absence of chlorid of calcium from the solutions, and this in the presence of excess of sulphates is attained simply by evaporating to the point where gypsum becomes insoluble. In nearly all river and spring waters bicarbonate of lime is present in a large proportion, and is often the most abundant salt. We have shown that when mingled with a solution containing sulphate of magnesia, it gives rise by double decomposition to bicarbonate of magnesia and sulphate of lime. By the evaporation of such a solution, the latter salt, being less soluble, is

first deposited in the form of gypsum, while the magnesian carbonate is only separated after further evaporation, when, provided the supply of bicarbonate of lime still continues, the two carbonates may fall down in a state of intermixture. In this way sediments will be formed containing the elements of dolomite of magnesite.

The solution of magnesian carbonate remaining after the deposition of the gypsum, possesses, as we have seen, the power of decomposing chlorid of calcium, and when deprived of a portion of its carbonic acid by evaporation, reacts in a similar manner with a solution of sulphate of lime. In this way, an influx of sea-water into the basin from which gypsum, and perhaps a portion of magnesian carbonate, has already been deposited, would give rise to a precipitate of carbonate of lime, like the tufaceous limestones, whose occurrence with gypsum and dolomites has been already noticed. In basins which, like the salt lagoons of Bessarabia on the shores of the Black Sea, receive occasional additions of sea-water, and deposit every summer large amounts of salt (Bischof, *Lehrbuch*, ii. 1717), the influx of waters containing bicarbonate of lime would give rise to the formation of beds of gypsum, alternating with dolomites or magnesian marls and rock salt.

We have already referred to the analyses of certain rivers, in which the sulphates are more abundant than the chlorids. Thus, in the Rhine, near Bonn, according to Bischof, we have for 100,000 parts of the water, 17·08 of solid matters, of which 1·23 are sulphate of lime, 1·81 sulphate of magnesia, with only 1·45 of chlorid and 8·37 of carbonate of lime; in the Danube near Vienna, the predominance of sulphates is still more marked. The waters of the Arve, in the month of February, gave to Tingry, for 100,000 parts, 24·5 of solid matters, of which 6·5 were sulphate of lime, 6·2 sulphate of magnesia, and 8·3 carbonate of lime, with only 1·5 of chlorids. Now, as in river waters there is always present an excess of carbonic acid, and as bicarbonate of lime and sulphate of magnesia in solution are mutually decomposed, these waters, which are to be regarded as solutions of sulphate of lime and bicarbonate of magnesia, would, by their evaporation, yield gypsum and magnesian carbonate, which would appear as portions of a fresh-water formation, like those of Aix and Auvergne.

The decomposition of soluble sulphates by bicarbonates of baryta and strontia, will explain the formation of heavy spar and celestine, and their frequent association with gypsiferous rocks.

As to the native sulphur which is often associated both with epigenic and sedimentary gypsums, it has doubtless in every case been formed, as Breislak long since indicated, by the decomposition of sulphuretted hydrogen. It is well known that alkaline and earthy sulphates are reduced to sulphurets by organic matters, with the aid of heat, or even at ordinary temperatures, in presence of water. To the decomposition of these sulphurets by water and carbonic acid, we are to ascribe not only the sulphuretted hydrogen of solfataras, which, by its oxydation, under different conditions, gives rise either to free sulphur, or to sulphuric acid and to gypsum by epigenesis, but also the sulphuretted hydrogen which appears in springs and in stagnant waters, where the sulphur produced by the decomposition of the gas is often mingled with sedimentary gypsums.* — See Bischof, *Lehrbuch*, ii. 139–185. This author has suggested the decomposition of chlorid of magnesium by alkaline or earthy sulphurets as a source of sulphuretted hydrogen and hydrate of magnesia, into which sulphuret of magnesium is readily resolved in the presence of water. — *Chem. Geology*, i. 16. If a salt of calcium were present, this reaction could only take place in the absence of carbonic acid, for carbonate of magnesia is incompatible with chlorid of calcium. The direct reduction and decomposition of sulphate of magnesia by organic matters and carbonic acid may, however, yield sulphuretted hydrogen and carbonate of magnesia, and thus, in certain cases give rise to magnesian sediments.

In the preceding sections, we have supposed the waters mingling with the solution of sulphate of magnesia to contain no other bicarbonate than that of lime, but bicarbonate of soda is often present in large proportion in natural waters, and the addition of this salt to sea-water or other solutions containing chlorids and sulphates of lime and magnesia, will, as we have seen, separate the lime as bicarbonate, and give rise to liquids, which, without being concentrated brines, as in the previous case, will contain sulphate of magnesia, but no lime salts. A further portion of bicarbonate of soda will produce bicarbonate of magnesia, by the evaporation of whose solutions as before, hydrated carbonate of magnesia would be deposited, mingled with the carbonate of lime which accompanies the alkaline salt, and in the case of the waters

* On certain modes of decomposition of the sulphates, see Jacquemin, *Comptes Rendus*, June 14, 1858.

of alkaline springs, the compounds of iron, manganese, zinc, nickel, lead, copper, arsenic, chrome, and other metals, which springs of this kind still bring to the surface. In this way the metalliferous character of many dolomites is explained, as also the frequent association of metals, such as copper, nickel, cobalt, chrome and titanium, with serpentine, steatite, diallage, olivine, and other magnesian silicates, which owe their origin to the alteration of magnesian sediments such as we have described.

As the separation of magnesian carbonate from saline waters by the action of bicarbonate of soda does not suppose a very great degree of concentration, we may conceive this process to go on in basins where animal life exists, and thus explain the origin of fossiliferous magnesian limestones like those of the Dudswell, and the Silurian rocks of the western United States, whose fossils, as I am informed by Mr. James Hall, of Albany, are generally such as indicate a shallow sea. To the intervention of carbonate of soda is, I conceive, to be referred the origin of all those dolomites which are not accompanied by gypsums, and which make up by far the larger part of the magnesian limestones; nor will the dolomites thus derived be necessarily marine, for the same reagent with waters like those of the Danube and Arve would give rise to dolomites and magnesites in fresh-water formations, which unlike those before mentioned, would not be accompanied by gypsums.

To the first stage of the reaction between alkaline bicarbonates and sea-water I am disposed to ascribe the formation of certain deposits of carbonate of lime which, although included in fossiliferous formations, are unlike most of their associated limestones, not of organic origin, but have the characters of a chemical precipitate of nearly pure carbonate of lime, in which are often imbedded silicified shells and corals.*

* The large proportion of dissolved silica which many river waters contain appears in sedimentary deposits, not only replacing fossils and forming concretions and even beds of flint, chert, and jasper, but also in a crystalline state, as is seen in the crystallized quartz often associated with these amorphous varieties, and in some beds of sandstone which are made up entirely of small crystals of quartz. Elie de Beaumont long since called attention to the crystalline nature of certain sandstones which, as Daubrée has remarked, could not have been derived from the disintegration of any known rock, and Mr. J. Brainard, at the meeting of the American Association for the Advancement of Science, held at Cleveland, insisted upon the crystalline character of the grains composing sandstones in Ohio, as evidence that these

It is not perhaps easy in all cases to distinguish between such precipitates, which may assume a concretionary structure (see on this question, Bischof, *Chem. Geology*, i. 428), and those deposits which like travertines have been formed from subterranean springs. In neither case, however, should they be confounded with the tufaceous limestones which we mentioned.

The union of the mingled carbonates of lime and magnesia to form dolomite, is attended with contraction, which in case the sediment was already somewhat consolidated, would give rise to fissures and cavities in the mass. Should the dolomitic strata be afterwards exposed to the action of infiltrating carbonated waters, the excess of carbonate of lime and any calcareous fossils would be removed, leaving the mass still more porous, with only the moulds of the fossils. Insoluble, however, as it appears to be at ordinary temperatures, the filling up of such cavities both in magnesian and in pure limestones, not less than its deposition in veins and druses, indicates that dolomite is, under certain conditions, soluble.

The lowest temperature at which hydrous magnesian sediments may be transformed into magnesite and dolomite has yet to be determined. The requisite heat has, however, doubtless been attained by the accumulation of overlying sediments, in virtue of that law which causes the

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were chemical deposits. He, however, fell into the error of supposing that all sandstones and even quartzose conglomerates have had a like origin, while the latter and the greater part of the former are undoubtedly mechanical deposits from the ruins of pre-existing quartzose and granitic rocks.

These crystallized sands, according to Daubrée, are met with in beds in the sandstone of the Vosges, the variegated sandstone (Triassic and Permian), in the tertiary of the Paris basin and elsewhere. Other sands are made up of globules of calcedony, apparently like the crystallized sands a chemical deposit, and associated with oolitic iron ores in the lias, and with glauconite grains in the green-sand. (Daubrée, *Recherches sur le Striage des Roches, etc.*, Ann. des Mines, 1857, 6 livr.) We may here mention the so-called *gaize* from the green-sand of the Ardennes, which gave to Sauvage 56·0 p. c. of amorphous soluble silica mixed with quartz sand and glauconite. (Bischof, *Lehrbuch*, i. 768–811).

Maschke has shown that under certain conditions silica is soluble in about forty parts of pure water; from this solution it separates by evaporation or by the addition of concentrated saline solutions in a form insoluble in water. (*Jour. für prakt. Chemie*, lxviii. 233.) In these reactions we have a key to the formation of silicious deposits.

temperature to increase as we penetrate the earth's crust. This increase we may suppose with Mr. Hopkins to have been much more rapid in former epochs than at present. — *Geol. Journal*, viii. 59, also Phillips' *Manual of Geology*, 609.

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## Note.

This paper forms part of an article published in the American Journal of Science for 1859 (Vol. xxviii.), in which will be found described with detail a series of chemical investigations, whose results may be summed up as follows:

1. The action of solutions of bicarbonate of soda upon sea-water separates in the first place the whole of the lime in the form of carbonate, and then gives rise to a solution of bicarbonate of magnesia, which by evaporation deposits hydrous magnesian carbonate.

2. The addition of solutions of bicarbonate of lime to sulphate of soda or sulphate of magnesia gives rise to bicarbonates of these bases, together with sulphates of lime, which latter may be thrown down by alcohol. By the evaporation of a solution containing bicarbonate of magnesia and sulphate of lime, either with or without sea salt, gypsum and hydrous carbonate of magnesia are successively deposited.

3. When the hydrous carbonate of magnesia is heated alone under pressure it is converted into magnesite, but if carbonate of lime be present, a double salt is formed, which is dolomite.

4. Solutions of bicarbonate of magnesia decompose chlorid of calcium, and when deprived of their excess of carbonic acid by evaporation, even solutions of gypsum, with separation of carbonate of lime.

5. Dolomites, magnesites, and magnesian marls, have had their origin in sediments of magnesian carbonate formed by the evaporation of solutions of bicarbonate of magnesia. These solutions have been produced by the action of bicarbonate of lime upon solutions of sulphate of magnesia, in which case gypsum is a subsidiary product; or by the decomposition of solutions of sulphate or chlorid of magnesium by the waters of rivers or springs containing bicarbonate of soda. The subsequent action of heat upon such magnesian sediments, either alone or mingled with carbonate of lime, has changed them into magnesite or dolomite.

## IV. PHYSICS OF THE GLOBE.

### 1. General Account of the Results of the Discussion of the Declinometer Observations made at Girard College, Philadelphia, between the Years 1840 to 1845, with Special Reference to the Eleven Year Period. By A. D. Bache, Superintendent of the U. S. Coast Survey.*

It is proposed to give here in outline the results of an investigation of the magnetic observations, made with the Declinometer between the years 1840 to 1845, at the Girard College observatory, with special reference to the eleven year period in the amplitude of the solar-diurnal variation and the disturbances of the magnetic declination. Prof. Henry, secretary of the Smithsonian Institution, has kindly offered to publish the memoir in full in the Smithsonian Contributions to Knowledge. It is my intention to pursue the discussion by taking up the investigation of the lunar influence on the same magnetic element.

In coöperation with the scheme adopted at the British colonial observatories, a series of magnetic and meteorological observations were made at the Girard College observatory with instruments purchased under the direction of the trustees of the college, the observations being made under the patronage of the American Philosophical Society, and finally completed for the use of the Topographical Bureau of the War Department. These observations were made under my direction and superintendence. The series commenced in May, 1840, and, with short interruptions, terminated in June, 1845; thus furnishing a five years' series of magnetic observations taken bi-hourly up to October, 1843, and after that date hourly. The readings of each magnetic element were united into means, and were also presented graphically (in the fourth volume of the record). This was done under my direction by J. Ruth, Esq., but owing to other laborious duties, the record could not be submitted to a more complete reduction. I have now resumed the subject by the assistance of Charles A. Schott, Esq., Assistant in

* Communicated to the American Association for the Advancement of Science, by authority from the Treasury Department.

the Coast Survey, by whom, under my immediate direction, and as my assistant in this special matter, the present paper has been prepared.*

Although the magnetic observations furnish, by their judicious geographical location, a basis for the generalization of their results, it is nevertheless desirable to obtain results from intermediate observatories as confirmations or as corrections. In the investigation of the disturbance law at Point Barrow, when compared with the same at Toronto, a very remarkable mutual relation of the law at these stations resulted from such a comparison, and further examination may bring to light other dependencies of a mutual character.

According to the latest determination, the position of the Girard College observatory is in latitude 39° 58′ 23″ (north), and in longitude 75° 10′ 05″ = $5^h$ $00^m$ $40^s.3$ west of Greenwich. From it Toronto bears 38° 45′ west of north (true) and is distant about 334 statute miles.

It is proposed specially to investigate the law of the eleven year period, or, as it is more frequently called, the decennial period, there being yet an uncertainty as to its precise length. It is supposed to have some direct or indirect connection with the solar spot period, which correspondence, according to late investigations by Prof. R. Wolf, is so close as to exhibit even analogous disturbances. The following discussion will afford a contribution towards the determination of the epoch of the occurrence of a minimum in certain phases of the magnetic variation and disturbances, corresponding to a minimum of the solar spots. The method of reduction is substantially the same as that adopted by General Sabine. Earlier investigations by Dr. Lamont and those by Mr. Kreil, differ from his in not including the discussion of the disturbances in connection with the period in question.

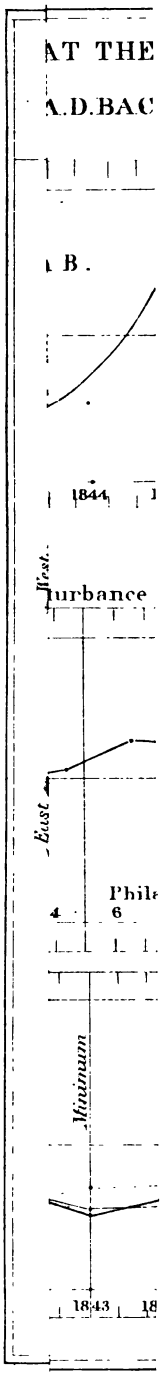
As long as the magnitude of the deflection remains the only criterion by which a disturbance may be recognized as such, the adoption of any limit of deviation from the normal value of the same hour, month, and year, must necessarily remain in some measure arbitrary, or, in other words, there must always remain after the separation of the disturbances, a certain small amount of their effect in the remaining regular diurnal progression. To effect the separation, Peirce's criterion has been

* It may be proper to state that this work has been performed out of office hours, and at my own expense.

used with entire success. After a preliminary investigation as to the number of disturbances separated, the limit, as pointed out by the criterion, or a deviation of 8 scale divisions (or 3′.6 of arc) has been adopted in the present discussion, as constituting a disturbed observation. Accordingly all observations differing by that amount or more from the mean monthly value of their respective hour were marked by a pencil line. Next a new hourly mean was taken, omitting values so marked, and each observation was again examined in reference to deviation from this new mean. This process was repeated when necessary, so that in all cases, values differing 8 scale divisions or more from the mean were excluded. The last mean thus obtained for each observing hour and each month has been called "the normal." These values have been tabulated and are given for each month and year separately, together with such corrections as the omissions or interruptions demanded. The bi-hourly and afterwards the hourly readings (and their means) were made $19\frac{1}{2}^{m}$ after the hour, so as to correspond to an even Göttingen hour.

For the purpose of comparing the annual means of the normals, or the mean march of the regular solar-diurnal variation for each year, the results have been expressed analytically by means of Bessel's formula, and by the application of the method of least squares.

Probably owing to the several accidental changes in the suspension of the bar, and consequent uncertainty in the precise amount of scale correction, the mean readings of each year, when compared with one another, exhibit differences not actually due to inequalities occasioned by declination changes. Though this question does not directly bear upon the present investigation, which mainly depends on differences of readings, it will be proper to remark that the observed increase giving the weight $\frac{1}{2}$ to the mean of 1840 and 1845 (on account of incomplete record) is, under the supposition of a uniform annual change between these years, equal to 4′.50. According to Mr. Schott's latest investigation of the secular change of the declination in Philadelphia, supported by observations between the years 1701 and 1855·7, the annual increase between the years 1840 to 1845 is 4′.98, a result which accords tolerably well with actual observations. According to this formula, the declination on the 1st of January, 1843, the mean epoch of the present series, is 3° 32′ west, with a probable error of $\pm$ 10′. This declination corresponds to the scale reading 560.31, which has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been deduced by taking into account the weights of the annual means.

The expressions have been thrown into curves (Diagram A), and the agreement between computed and observed values is shown by the introduction of dots giving the observed reading. The probable error of any single representation is $\pm$ 0′.1. By means of the formulæ the following values were computed:

| For | Epoch of maximum eastern deflection. | Corresponding scale reading. | Epoch of maximum western deflection. | Corresponding scale readings. | Amplitude in arc. |
|---|---|---|---|---|---|
| | h. m. | d. | h. m. | d. | |
| 1840 | 7 26 A. M. | 595.67 | 1 34 P. M. | 575.71 | 9′.08 |
| 1841 | 7 49 | 477.96 | 1 49 | 560.21 | 8.06 |
| 1842 | 7 36 | 571.24 | 1 37 | 553.96 | 7.83 |
| 1843 | 7 40 | 569.54 | 1 24 | 553.06 | 7.46m |
| 1844 | 7 32 | 556.50 | 1 18 | 539.99 | 7.51 |
| 1845 | 7 34 | 536.65 | 1 16 | 517.81 | 8.53 |
| Mean, | 7 36 A. M. | | 1 30 P. M. | | |
| | $\pm$ 3m | | $\pm$ 4m | | |

The inequality constituting the ten or eleven year period is plainly exhibited in the last column, the progression of the numbers being quite regular; the year 1843 is directly indicated as the year of the minimum range of the diurnal fluctuation. By means of a special formula, deduced by least squares, and representing a single value by $\pm$ 0′.11, the month of May, 1843, is indicated as the epoch of minimum amplitude. (Diagram B.)

The discussion of the disturbances, as far as they bear on the decennial inequality, next follows, taking in also some collateral results.

The total number of observations for changes of declination recorded and discussed amount to 24,566; of these, according to the preceding investigation, 2,357 were separated as disturbances. There is one disturbed observation in every 10.4 observations. The discussion of the disturbances are divided into two parts, that of the number, and that of the amount of the deflections. Omissions in the record have been supplied by the use of proper ratios showing the law as given by the full periods; and interpolated values are enclosed by brackets. The number of disturbances in each month of the year or the annual inequality in the distribution of the disturbances has been made out for each year, and the means and ratios are also given. The principal maximum oc-

curs in October (at Toronto in September), the secondary in April; the two minima, nearly equal in amount, occur in February and June. (The first one in January at Toronto.) The ratios of the number of monthly disturbances to the average number are given in the following table showing the same, also divided into westerly and easterly values.

| | RATIOS. | | Total. | | RATIOS. | | Total. |
|---|---|---|---|---|---|---|---|
| | W. | E. | | | W. | E. | |
| January, | 1.27 | 0.42 | 0.77 | July, | 0.58 | 1.18 | 0.86 |
| February, | 0.70 | 0.46 | 0.52 m | August, | 2.00 M | 1.14 | 1.59 |
| March, | 0.83 | 0.57 | 0.68 | September, | 0.93 | 1.86 | 1.36 |
| April, | 0.95 | 0.80 | 0.91 $M_2$ | October, | 1.58 | 2.50 M | 2.12 M |
| May, | 0.55 | 0.60 | 0.58 | November, | 0.96 | 1.12 | 1.08 |
| June, | 0.44 | 0.61 | 0.53 m | December. | 1.21 | 0.74 | 1.00 |
| | | | | Mean, | 1.00 | 1.00 | 1.00 |

The ratios show a general correspondence in the numbers of westerly and eastern deflections. The westerly seem to occur most frequently in August, while the easterly predominate in October; the secondary maximum of both series is in April. For the total number, the minima occur in February and June. The following table contains the number of disturbances in each year.

| | | | |
|---|---|---|---|
| 1840 | 483 | Weight, $\frac{1}{2}$. | Proportional number of western disturbances 937, of eastern 912; at Toronto the eastern predominate over the western in the proportion of 1.17 to 1. |
| 1841 | 539 | | |
| 1842 | 446 | | |
| 1843 | 275 | Weight, $\frac{2}{3}$. | |
| 1844 | 308 | | |
| 1845 | 264 | Weight, $\frac{1}{2}$. | |

These numbers do not indicate the law of the eleven year period as plainly and systematically as found by the investigation of the diurnal amplitude, yet giving proper weight (on account of deficiencies), the minimum number of disturbances falls in the year 1842.

If we distribute the disturbances, 1,943 in number, for the even hours, according to their respective hours of occurrence, we find the following ratios:

| Philadelphia mean time. | RATIOS. | | Total number. |
|---|---|---|---|
| | W. | E. | |
| h. m. | | | |
| 0 19½ | 0.82 | 1.20 | 162 |
| 2 " | 1.18 | 1.16 | 189 M. |
| 4 " | 1.08 | 0.96 | 168 |
| 6 " | 1.35 | 0.80 | 173 |
| 8 " | 1.29 | 0.70 | 161 |
| 10 " | 1.32 | 0.88 | 178 |
| Noon " | 1.13 | 0.71 | 150 |
| 14 " | 0.95 | 0.67 | 133 m. |
| 16 " | 1.07 | 0.78 | 148 |
| 18 " | 0.87 | 0.90 | 143 |
| 20 " | 0.40 m. | 1.66 M. | 167 |
| 22 " | 0.54 | 1.58 | 170 |

The numbers in each column show a regular progression; the disturbances, irrespective of their direction, have a minimum at 2 P. M., and a maximum at 2 A. M. (At Toronto the respective hours are 2 P. M., and 22 P. M.) The principal contrast is between the hours of the day and the hours of the night. In the table given above, the most striking result is that the westerly disturbances have their minimum precisely at the hour (8 P. M.) when the easterly have their maximum, and the exact coincidence of this result with that deduced by General Sabine for Toronto is not less remarkable. In connection with this subject, it may be remarked, that the same distinguished magnetist found a singular mutual relation to subsist between the phenomena at Toronto and Point Barrow, on the shore of the Arctic sea, — the law of the easterly deflection at one station being found to correspond at the same local hours with those of the westerly deflections at the other station, and *vice versa*. This contrast, therefore, holds good for Philadelphia as well as Toronto.

If we classify the disturbances according to their amount, we obtain the total aggregate and mean values of a single disturbance in the different years, as follows:

| | Aggregate amount in scale divisions. | Mean value. | Mean value. | |
|---|---|---|---|---|
| | | | W. | E. |
| | d. | ′ | ′ | |
| 1840 | 7155.5 | 6.70 | 6.52 | 7.20 |
| 1841 | 7844.4 | 6.61 | 5.93 | 7.07 |
| 1842 | 6019.1 | 6.11 | 5.53 | 6.70 |
| 1843 | 2932.2 | 4.85 | 4.85 | 4.85 |
| 1844 | 4227.3 | 6.21 | 6.21 | 6.21 |
| 1845 | 3521.4 | 6.02 | 5.25 | 6.84 |

The eleven year period is well marked in the aggregate, as well as in the mean values, and the precise epoch of the minimum was found by a special formula. It took place in August, 1843, and as a resulting epoch from this and the previous determination, June, 1843, may be adopted. This is graphically represented on diagram C.

The following table gives the ratios of the aggregate amount of disturbances in each month of the year.

| | Total. | W. | E. | | Total. | W. | E. |
|---|---|---|---|---|---|---|---|
| January, | 0.72 | 1.21 | 0.33 | July, | 0.87 | 0.46 | 1.29 |
| February, | 0.54 $m_2$ | 0.59 | 0.46 | August, | 1.61 | 1.95 | 1.28 |
| March, | 0.66 | 0.73 | 0.56 | September, | 1.56 | 0.99 | 2.12 |
| April, | 0.94 $M_2$ | 1.04 | 0.84 | October, | 2.06 $M_1$ | 1.67 | 2.46 |
| May, | 0.56 | 0.52 | 0.56 | November, | 1.06 | 1.09 | 1.02 |
| June, | 0.42 $m_1$ | 0.37 | 0.47 | December, | 1.00 | 1.38 | 0.61 |

In reference to the first column, the maximum amount of disturbances occurs in October (at Toronto in September); the minimum in June (as at Toronto); the secondary maximum occurs in April (the same at Toronto); the secondary minimum occurs in February (and at Toronto in January). If separated into west and east deflections, maxima occur in September (mean of August and October) and April, and minima in June and January (same as at Toronto).

The arrangement according to the hours of the day gives the following ratios, to which is added, in the last column, the diurnal disturbance variation obtained by dividing the excess of aggregate westerly over easterly values by the total number of days of observation.

| Philadelphia mean time. | RATIOS. | | | Disturbance variation in arc. |
|---|---|---|---|---|
| | W. | E. | Both combined. | |
| h. m. | | | | ′ |
| 0 (19½) | 0.83 | 1.24 | 1.04 | — 0.16 |
| 2 " | 1.16 | 1.10 | 1.13 | — 0.01 |
| 4 " | 1.16 | 0.92 | 1.04 | + 0.05 |
| 6 " | 1.46 | 0.67 | 1.06 | + 0.24 |
| 8 " | 1.39 | 0.67 | 1.02 | + 0.22 |
| 10 " | 1.22 | 0.77 | 0.99 | + 0.13 |
| Noon " | 1.03 | 0.63 | 0.83 | + 0.11 |
| 14 " | 0.98 | 0.63 | 0.80 m | + 0.10 |
| 16 " | 0.99 | 0.72 | 0.85 | + 0.07 |
| 18 " | 0.84 | 0.89 | 0.84 | — 0.02 |
| 20 " | 0.38 m | 1.88M | 1.15 | — 0.53 |
| 22 " | 0.56 | 1.88 | 1.25 M | — 0.47 |

If we compare these ratios with the corresponding number in a preceding table showing the bi-hourly distribution in regard to number, we find, irrespective of the direction of the deflections, the 2 P. M., minimum preserved. The maximum is earlier, and occurs at 10 P. M. (At Toronto these hours are respectively 1 P. M., and 9 P. M.). At Philadelphia as well as at Toronto the ratios are nearly invariable from 10 A. M., to 6 P. M., and again from 8 P. M., to 8 A. M. The easterly maximum and westerly minimum at 8 P. M. form a marked feature.

The law governing the disturbances during a solar day is clearly shown, and is of a systematic character. The diurnal variation caused by the disturbances, if superposed on the regular diurnal variation, would produce what is known by the name "mean diurnal variation." If we plot the disturbance curve on the same scale, or actually superpose it on the curves of the regular diurnal variation, it would hardly show to the eye, and the compound curve of the mean variation would keep within the maximum distance of a division from the regular curve in the diagram (D). The disturbance variation has but one maximum and one minimum. Its most prominent feature, is the easterly deflection at eight o'clock ($+19\frac{1}{2}^{m}$) P. M. (at Toronto it is at 9 P. M.); the maximum deflection amounts at that hour to 32″ of arc (and to 45″ at Toronto). The greatest westerly deflection occurs at $6^{h}$ ($19\frac{1}{2}^{m}$) A. M., and amounts to but 14″. (At Toronto the hour is 8 A. M., with 6″, and from a five years' series of observation, with 31″ of deflection.) The range of the disturbance variation equals 46″. From three in the

morning till five in the afternoon, the mean effect of the disturbances is to deflect the north end of the magnet to the west, and during the remaining hours (principally night hours), to the east. The westerly and easterly disturbance deflections, during a day, balance within 0′.02.

The annual inequality in the amplitude of the diurnal disturbance variation cannot satisfactorily be shown, on account of the short and partly interrupted series of observations.

It is my intention to continue the discussion of the observations made at the Girard College observatory.

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After the above was written, No. 1185 of the Astronomische Nachrichten came to hand, containing Prof. R. Wolf's interesting results on the close connection of the variation in the frequency of the solar spots, and the corresponding inequality in the amplitude of the diurnal variation of the declination. He deduces for Munich the simple formula $\beta = 6'.273 + 0'.051\ \alpha$, where $\alpha$ represents a relative number expressive of the frequency of the solar spots, directly derived from observation, and $\beta$ the amplitude of the diurnal variation. He found a close correspondence between these phenomena, showing the observed and computed amplitude for the Munich observations between 1835 and 1850. The average length of the solar spot period is reaffirmed to be 11.11 years $\pm$ 0.04 years. For Philadelphia we obtain $\beta = 7'.080 + 0'.039\ \alpha$, which formula represents the observation as follows.

| Year. | $\alpha$ from solar spot observations. | Amplitude derived from $\alpha$. | Observed amplitude. | Difference. |
|---|---|---|---|---|
| | | ′ | ′ | ′ |
| 1840 | 51.8 | 9.10 | 9.08 | —0.02 |
| 1841 | 29.5 | 8.23 | 8.06 | —0.17 |
| 1842 | 19.2 | 7.83 | 7.83 | 0.00 |
| 1843 | 8.4 | 7.41 | 7.46 | +0.05 |
| 1844 | 12.2 | 7.55 | 7.51 | —0.04 |
| 1845 | 32.4 | 8.34 | 8.53 | +0.19 |

The correspondence between the observed diurnal amplitude, and the same computed from observations of the solar spots is further exhibited in the annexed diagram (E). The dotted curve is the approximate Toronto curve from observation, especially introduced to show the agreement at the epoch of the maximum in 1848. By computation

from the solar spot observations, the amplitude at that time would amount to 11′.00, the whole range of the inequality of the diurnal variation would therefore be 11′.00 — 7′.46 = 3′.54.

It is much to be desired that this interesting branch of physical inquiry be further studied, as it forms one of the links connecting terrestrial with cosmical phenomena.

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2. GULF STREAM EXPLORATIONS — THIRD MEMOIR. — DISTRIBUTION OF TEMPERATURE IN THE WATER OF THE FLORIDA CHANNEL AND STRAITS. By A. D. BACHE, Superintendent U. S. Coast Survey.*

The results of the explorations of the Gulf Stream in the survey of the coast, have been communicated to the Association from time to time, as phenomena of peculiar interest have been developed. The original plan of these explorations having been carefully studied and having proved successful, has steadily been adhered to. The more recent observations have been directed to that part of the stream between Havana and Cape Florida, known as the channel and strait of Florida.

I have now to present four sections, showing the depth and temperature in this most important region of the Gulf Stream. These results are from the observations of Commander B. F. Sands, and Lieut. Comg. T. A. Craven, U. S. Navy, Assistants in the Coast Survey, whose names have already been mentioned before the Association in connection with explorations of the Gulf Stream, and furnish a sufficient guaranty that the results have all the reliability which care, experience, and zealous labor could give them.

SECTION No. 1, from Cape Florida to Bemini, was run by Lieut. Comg. Craven in May, 1855; section No. 4 by Commander Sands, in May, 1858; and sections Nos. 2 and 3 by Lieut. Comg. Craven, in April and May of the present year — 1859. Sections 2, 3, and 4 are perpendicular to the direction of the stream at distances of about

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* Communicated by authority of the Treasury Department.

fifty, one hundred, and two hundred miles from Cape Florida. The lines of sections are shown upon the chart marked 4. The Florida strait is funnel-shaped, being about ninety miles wide at Havana, and about forty-five miles wide at Cape Florida—the narrowest part.

### *Form of Bottom.*

The area of the water way and the form of the bottom are represented on diagrams 7, 8, 9, and 10. The Arabic numerals at the top represent distances from the Florida coast (the Keys) in miles, and the Roman numerals the positions at which observations were made. The numbers at the left hand represent the depth in fathoms.

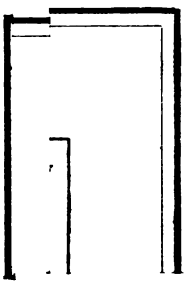
Commencing at the Cape Florida section, it will be seen that there is a descent of the bottom to the Havana section, from three hundred and fifty fathoms to eight hundred fathoms, or twenty-seven hundred feet in a distance of two hundred miles. The most shallow as well as the narrowest part of the stream is, therefore, at Cape Florida. The deepest water follows the coast of Cuba and the Grand Banks, the depth being eight hundred fathoms at a distance of only five miles from Havana, nearly four hundred fathoms within five miles of Salt Key Bank, and three hundred fathoms close to the island of Bemini. The descent from the Florida side is, for the most part, gradual, but from the opposite side abrupt. This effect seems to have been produced by the action of the sub-current in wearing a deeper channel upon the concave side of the stream.

At Havana there is an abrupt descent of nearly a mile within five miles of the shore, while on the side of the Tortugas and Key West, the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf Stream makes the circuit of the Gulf of Mexico, since, if it impinged directly upon the islands of Key West and the Tortugas, we should find its effects in the wearing of a deeper channel on that side.

## TEMPERATURES.

### *Change of Temperature with Depth.*

In a former communication the law of change of temperature with depth was discussed, and types of the curves representing the law were



given for different parts of the stream. These curves were all nearly modifications of a more general form. Thus, the cold water between the Gulf Stream and the coast gave one form; the axis of the stream another; and the water beyond the axis a third form: while in the strait of Florida a fourth was developed. It would be natural to expect in the course of many years' explorations by different individuals with different instruments, not even of the same class, that general phenomena of this character should present some contradictions and some inexplicable results. Experience, however, has confirmed the first conclusions and the constancy of the phenomena. It is not difficult, having the curve representing the temperatures at any position from the surface to the depth of several hundred fathoms, to determine from the temperatures alone, in what part of the stream they were taken.

*Temperature in a direction perpendicular to the Stream.*

Diagrams 2, 3, 4, and 5, show the changes of temperature for the same depth in each of the sections, and diagrams 7, 8, 9, and 10, the depth for the same temperature.

*Bands of warm and cool Water.*

In the section from Cape Florida to Bemini, the division of the stream into bands is plainly exhibited, though more faintly than in the northern sections, and the form of the bottom in this section shows also elevations and depressions corresponding to the divisions. In the sections south of Cape Florida, all traces of the bands seem to disappear as well as the ridges of the bottom. The bands, therefore, seem to have their origin near Cape Florida, and the conclusion stated some years ago as the probable one is strengthened, that they are caused by the ridges and valleys of the bottom parallel to the general course of the stream, and along which the stream and counter-stream have their course.

*The Cold Wall.*

The cold wall, as an exception to the remark made above in reference to the bands, is traced as far as the Tortugas, and is plainly shown in all the sections with more or less distinctness. In the Sombrero Key section (No. 3) it is strongly marked at depths ranging from

seventy to a hundred fathoms, while in all the sections the warm water at the surface overflows the cold wall and reaches quite to the shore.

Diagram No. 6 represents the comparative curves of the cold wall in different sections of the Gulf Stream, including those in the straits of Florida. The figures at the top show the distances of the cold wall from the shore in the different sections, and the numbers on the left the degrees of temperature. The curves are drawn for different depths in the several sections, as shown in the notes at the bottom of the diagram. The curves *g*, *h*, *i*, *k* represent the cold wall in the four sections under consideration.

*Longitudinal Sections.*

It has been found very difficult to deduce any satisfactory law for the decrease of surface temperature along the axis of the stream owing to the variability of the temperature of the waters of the regions from whence the Gulf Stream is supplied.

Two modes of investigating the subject have been pursued, one by following the stream from the Gulf of Mexico and making hourly observations of the temperature of the water, and the other by comparing the mean temperatures of the various sections with each other, and with the temperature of the Gulf of Mexico. In the first method, the vessel must be allowed to drift with the current of the stream, a difficult condition except in the best weather, even for a day, and to float along thus for hundreds of miles would rarely be practicable. Any motion communicated by sails or by steam must carry the vessel beyond the water in which she commenced her voyage, and the lateral overflow carries the water constantly from the axis towards the edges of the stream.

In the comparison of mean temperatures of the different sections, the fact has been established, that the temperature of the water of the stream at any point, may be higher than at a point nearer the source, and hence, vessels in running along the stream may and generally do pass through water not of a constantly diminishing temperature, but from cool to warm, and the reverse. This is to be explained mainly, though not entirely, by the variability of temperature at the source. By taking the mean temperature of any one section, and going back to the date of the departure of the waters from the Gulf of Mexico, as

determined by the velocity of the stream, and comparing the temperatures observed, with the temperature of the Gulf waters, it was supposed that a solution of the question might be obtained. The temperatures were taken from the most authentic meteorological records of the Gulf for a series of years, and those periods sought which corresponded to the dates desired. The uncertainty of the temperatures of the waters of the Gulf of Mexico, as obtained from air temperatures taken here and there along its shores, rendered the results unsatisfactory. Enough seems to have been determined, however, to show that the surface temperature of the Gulf Stream along its course is variable; that a vessel sailing along the axis at a more rapid rate than the motion of the stream will pass through water of higher and lower temperature, depending generally upon two conditions, namely, the distance from the Gulf of Mexico, and the temperature of the Gulf at the time the water entered the straits of Florida; and further, that the latter cause is the predominating one in the parts of the Gulf Stream adjacent to the Atlantic coast where the current is rapid.

The influence of the form of the bottom in forcing the cold countercurrent of the bottom upward, has been adverted to, and the fact appears to be well established in the cross sections where the ridges and valleys parallel to the direction of the stream separate it into bands of warmer and cooler water, and this conclusion, as has just been stated, is strengthened by the fact that the bands and ridges simultaneously disappear south of Cape Florida. This phenomenon is moreover strikingly exhibited in the longitudinal section of the bottom, in connection with the lower temperatures.

The shallowness of the stream in the strait of Florida, connected with the fact that the bottom falls off rapidly to the north and south, afforded an excellent opportunity for testing this question. If the cold water of the under polar current follows the bottom, it should appear in the shallow part of the strait, and here the warm water of the surface, and the cold water of the bottom, would approach each other. Diagram No. 1 shows the curves of 40°, 45°, and 50° (bottom temperatures) along the deepest part of the stream, commencing at Sandy Hook, and running as far as the Tortugas. All these curves rise with the bottom and pass over the ridge which divides the bed of the Atlantic from that of the Gulf of Mexico, and again fall with the slope of the bottom towards the Gulf. In the narrowest part of the stra

where the depth is three hundred and fifty fathoms, the temperature from the surface to the bottom, ranges between 80° and 40°.

*On the Effects of Pressure on Saxton's Deep-Sea Thermometer.*

In the exploration of the Gulf Stream, the temperatures below one hundred fathoms have mostly been determined by Saxton's metallic thermometer, and although the results have been consistent among themselves, and have agreed well with the indications of other thermometers, yet it was thought advisable to determine the effect of pressure by direct experiment.

Saxton's thermometer consists essentially of a compound ribbon of silver and platinum fused and pressed together by rollers. This ribbon is wound in a spiral form, one end of the spiral being firmly fastened to an interior solid axis and the other left free. Upon the free end is placed an index arm which moves over a circular graduated scale carrying with it a friction hand or indicator which is left at the extreme point of the arc reached by the true index. The instrument is enclosed in a case to which the water is freely admitted. A variation of temperature is immediately noticed, as the effect is to give a rotary motion to the index.

The experiments to determine the effect of pressure were made at my request by Mr. J. M. Batchelder with means devised by Mr. Thomas Davison at the Novelty Iron Works. The following description of the apparatus employed is given by the last named gentleman.

"The gauge consists of a brass cylinder *H*, about eight inches long, into which a steel plunger is fitted, the upper part of the plunger at *A* being .70 of an inch in diameter, and the lower at *B* about .786, so that the difference in area of the ends is equal to one tenth of a square inch. The cylinder is bored out a little larger than the plunger except for about a fourth of an inch near each end at *C* and *D* where both are accurately fitted. To the branch *E* a pipe connects, communicating with the hydraulic cylinder and leading the water into the centre of the gauge, which it reaches after passing through the chamber *F* filled with sponge to prevent any impurities in the water from reaching the plunger. The upper end of the plunger connects by a wire *W*, to a spring as shown in the sketch at *G*, so constructed as to indicate pressure from 0 to 455 lbs., the spring being so strong that 450 lbs. produce

a movement of the plunger equal to three eighths of an inch. It is evident that as the difference in area of the ends of the plunger is one tenth of an inch, one hundred pounds pressure from the water on this surface, as indicated by the balance, would equal a pressure of water of one thousand lbs. per inch, or a pressure ten times as great as that indicated by the balance throughout its scale. The only difficulty in the use of the gauge is that of fitting the plunger to the cylinder so that while it is perfectly free to move, it is also water tight. This difficulty however has been overcome, and much advantage was also derived from Mr. Batchelder's suggestion for supplying the wear of the plunger and cylinder by depositing brass on the plunger through the galvanic process."

Connected with this gauge by a pipe is a strong wrought iron cylinder, sixteen inches long by four inches in diameter, in which the thermometer was placed, the opening being firmly closed by a screw plug. This second cylinder was immersed in a tub of water for the purpose of regulating the temperature. The thermometer once placed in the cylinder, is not again removed, the index being read by means of a mirror until the observations are completed.

By the use of this apparatus the effect of pressure up to four thousand lbs. per square inch was observed upon two thermometers, and the results are given below. The observations were made to indicate the effects of 500, 1,000, 1,500, 2,000, 2,500 lbs. pressure, etc. Seven series of experiments were made with thermometer No. 5, and five series with No. 10. The mean results show that a pressure of 1,000 lbs. per square inch has no effect upon the thermometer; at 1,500 lbs. the effect is less than one degree; and from 1,500 to 4,000 lbs. per square inch, the effect is to diminish the readings — the maximum effect being seven degrees.

The diagram exhibits the law of diminution by increase of pressure, and the depth corresponding to different pressures. The correction to be applied varies with the depth. For thermometer No. 5 it is only four tenths of a degree Fahrenheit at the depth of 600 fathoms. For thermometer No. 10, it is one degree at the same depth. At 1,500 fathoms the corrections are respectively five and a half and seven degrees.

Nearly all the temperatures observed in the Gulf Stream have been taken at depths less than 600 fathoms.

TABLE, SHOWING DIFFERENCES OF READINGS OF SAXTON'S THERMOMETER UNDER PRESSURE, AND FREE FROM PRESSURE.

*Thermometer No.* 5.

| No. of series. | PRESSURE IN POUNDS. | | | | | |
|---|---|---|---|---|---|---|
| | 1500 | 2000 | 2500 | 3000 | 3500 | 4000 |
| 1 | 1°.6 | 0°. | 3°.75 | 0°. | 0°. | 0°. |
| 2 | 0. | 1.00 | 2. | 2.8 | 4.5 | 5.5 |
| 3 | 0. | 1. | 2.25 | 3.75 | 4.75 | 5.7 |
| 4 | 0. | 0.5 | 0.50 | 2.0 | 3.6 | 5.5 |
| 5 | 0. | 1.75 | 2.25 | 3.5 | 5.0 | 6.5 |
| 6 | 0. | 1.25 | 2.25 | 3.75 | 6.0 | 6.5 |
| 7 | 0.5 | | | | 3.5 | 4.25 |
| Means. | 0.3 | 1.1 | 2.1 | 3.2 | 4.5 | 5.6 |

*Thermometer No.* 10.

| No of series. | PRESSURE IN POUNDS. | | | | | |
|---|---|---|---|---|---|---|
| | 1500 | 2000 | 2500 | 3000 | 3500 | 4000 |
| 1 | 1°.0 | 2°.0 | 3°.25 | 4°.5 | 6°.25 | 8°.25 |
| 2 | 2.0 | 1.0 | 3.5 | 4.5 | 6.0 | 7.25 |
| 3 | 0.75 | 2.0 | 3.0 | 3.25 | 5.5 | 6.5 |
| 4 | 1.75 | 2.0 | 3.5 | 4.75 | 5.5 | 7.25 |
| 5 | .75 | 1.75 | 1.75 | 3.75 | 5.0 | 6.75 |
| Means. | 1.00 | 1.75 | 3.0 | 4.25 | 5.6 | 7.25 |

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3. ABSTRACT OF THE PRINCIPAL RESULTS OF THE OBSERVATIONS FOR TEMPERATURE, AT VAN RENSSELAER HARBOR, NORTH GREENLAND, MADE BY THE SECOND GRINNELL EXPEDITION, UNDER COMMAND OF DR. E. K. KANE, U. S. N., DURING 1853–'54–'55. Presented by Prof. A. D. BACHE, from a reduction and discussion by CHAS. A. SCHOTT, Assistant Coast Survey.*

THE series of hourly observations commenced with September, 1853, and is continued with but few exceptions to January 24, 1855; monthly

* For the complete paper, see Smithsonian Contributions to Knowledge, Vol. XI.

TABLE, SHOWING DIFFERENCES OF READINGS OF SAXTON'S THERMOMETER UNDER PRESSURE, AND FREE FROM PRESSURE.

*Thermometer No.* 5.

| No. of series. | PRESSURE IN POUNDS. | | | | | |
|---|---|---|---|---|---|---|
| | 1500 | 2000 | 2500 | 3000 | 3500 | 4000 |
| 1 | 1°.6 | 0°. | 3°.75 | 0°. | 0°. | 0°. |
| 2 | 0. | 1.00 | 2. | 2.8 | 4.5 | 5.5 |
| 3 | 0. | 1. | 2.25 | 3.75 | 4.75 | 5.7 |
| 4 | 0. | 0.5 | 0.50 | 2.0 | 3.6 | 5.5 |
| 5 | 0. | 1.75 | 2.25 | 3.5 | 5.0 | 6.5 |
| 6 | 0. | 1.25 | 2.25 | 3.75 | 6.0 | 6.5 |
| 7 | 0.5 | | | | 3.5 | 4.25 |
| Means. | 0.3 | 1.1 | 2.1 | 3.2 | 4.5 | 5.6 |

*Thermometer No.* 10.

| No. of series. | PRESSURE IN POUNDS. | | | | | |
|---|---|---|---|---|---|---|
| | 1500 | 2000 | 2500 | 3000 | 3500 | 4000 |
| 1 | 1°.0 | 2°.0 | 3°.25 | 4°.5 | 6°.25 | 8°.25 |
| 2 | 2.0 | 1.0 | 3.5 | 4.5 | 6.0 | 7.25 |
| 3 | 0.75 | 2.0 | 3.0 | 3.25 | 5.5 | 6.5 |
| 4 | 1.75 | 2.0 | 3.5 | 4.75 | 5.5 | 7.25 |
| 5 | .75 | 1.75 | 1.75 | 3.75 | 5.0 | 6.75 |
| Means. | 1.00 | 1.75 | 3.0 | 4.25 | 5.6 | 7.25 |

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3. ABSTRACT OF THE PRINCIPAL RESULTS OF THE OBSERVATIONS FOR TEMPERATURE, AT VAN RENSSELAER HARBOR, NORTH GREENLAND, MADE BY THE SECOND GRINNELL EXPEDITION, UNDER COMMAND OF DR. E. K. KANE, U. S. N., DURING 1853–'54–'55. Presented by Prof. A. D. BACHE, from a reduction and discussion by CHAS. A. SCHOTT, Assistant Coast Survey.*

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November, and a maximum (9°.09′) in April. The mean diurnal amplitude is 4°.20 for the whole year; and on the average amounts still to 1°.6 during the long arctic night. (Diagram No. 2.) The following table shows the mean diurnal variation during the whole year.

| Hour. | Diurnal variation. | Hourly difference. | Hour. | Diurnal variation. | Hourly difference. |
|---|---|---|---|---|---|
| | ° | | | ° | |
| 1 | — 1.79 | ° | 13 | + 1.87 | ° |
| | | — 0.05 | | | — 0.03 |
| 2 | — 1.74 | | 14 | + 1.90 | + 0.22 |
| | | 0.03 | | | |
| 3 | — 1.71 | | 15 | + 1.68 | 0.28 |
| | | 0.16 | | | |
| 4 | — 1.55 | | 16 | + 1.40 | 0.29 |
| | | 0.27 | | | |
| 5 | — 1.28 | | 17 | + 1.11 | 0.38 |
| | | 0.38 | | | |
| 6 | — 0.90 | | 18 | + 0.73 | 0.44 |
| | | 0.45 | | | |
| 7 | — 0.45 | | 19 | + 0.29 | 0.45 |
| | | 0.62 | | | |
| 8 | + 0.17 | | 20 | — 0.16 | 0.40 |
| | | 0.57 | | | |
| 9 | + 0.74 | | 21 | — 0.56 | 0.46 |
| | | 0.32 | | | |
| 10 | + 1.06 | | 22 | — 1.02 | 0.39 |
| | | 0.37 | | | |
| 11 | + 1.43 | | 23 | — 1.41 | 0.23 |
| | | 0.38 | | | |
| Noon | + 1.81 | | Mdn't | — 1.64 | |
| | | — 0.06 | | | + 0.15 |

— below } the mean.
+ above }

The mean diurnal variation has been expressed as follows: — $t = + 1°.85 \sin (\theta + 65°) + 0°.08 \sin (2\theta + 97°) + 0°.03 \sin (3\theta + 128°) \pm 0°.03$. The angle $\theta$ is reckoned from noon. The hours of the day when the mean temperature is reached, as well as the critical interval.

| Month. | Morning hour. | Evening hour. | Critical interval. |
|---|---|---|---|
| | | | h. |
| September, | 7.4 | 18.4 | 11.0 |
| October, | Uncertain, probably one near 3h, the other near 9h. | 17.1 | 11.1 |
| November, | | Uncertain, one near 15h, the other near 21h. | |
| December, | | | 12.0 |
| January, | | | |
| February, | 8.9 | 21.4 | 12.5 |
| March, | 8.5 | 19.1 | 10.6 |
| April, | 8.4 | 20.7 | 12.3 |
| May, | 6.9 | 20.2 | 13.3 |
| June, | 6.7 | 20.7 | 14.0 |
| July, | 7.6 | 19.1 | 11.5 |
| August. | 7.9 | 19.8 | 11.9 |
| Means, | 7.2 | 19.2 | 12.0 |

Likewise the absolute hourly, daily, and monthly changes have been tabulated for each month of the year.

| | Hourly. | Daily. | Monthly. | | Hourly. | Daily. | Monthly. |
|---|---|---|---|---|---|---|---|
| | ° | ° | ° | | ° | ° | ° |
| Sept. | 7.0 | 15.6 | 32.3 | June, | 8.0 | 15.0 | 24.2 |
| Oct. | 8.0 | 24.1 | 41.0 | July, | 8.0 | 14.6 | 23.0 |
| Nov. | 10.8 | 25.0 | 42.8 | Aug. | 8.0 | 20.8 | 29.8 |
| Dec. | 19.4 | 30.4 | 60.5 | Sept. | 16.0 | 24 0 | 36.0 |
| Jan. | 8.1 | 21.8 | 69.8 | Oct. | 11.0 | 25.0 | 61.0 |
| Feb. | 12.3 | 37.8 | 56.4 | Nov. | 11.0 | 32.0 | 54.9 |
| March, | 13.5 | 24.1 | 56.7 | Dec. | 16.7 | 41.1 | 52.9 |
| April, | 10.0 | 33.1 | 56.7 | Jan. | 18.9 | 35.6 | 81.6 |
| May, | 7.8 | 17.8 | 46.5 | | | | |
| | | | | Means, | 11.4 | 25.7 | 48.6 |

The absolute minima of temperature observed were — 66°.4 on February 5, 1854, and — 65°.5 on January 8, 1855. The absolute maximum (in shade) was + 51°.0 on July 23, 1854.

The mean monthly temperatures from September, 1853, to April, 1855, both inclusive, were next tabulated.

| | | ° | | | ° |
|---|---|---|---|---|---|
| 1853–1854, | Sept. | + 13.45 | 1854–1855, | March, | — 34.88 |
| | Oct. | — 3.58 | | April, | — 10.35 |
| | Nov. | — 21.95 | 1854, | May, | + 13.45 |
| | Dec. | — 31.12 | | June, | + 30.12 |
| 1854–1855, | Jan. | — 28.22 | | July, | + 38.19 |
| | Feb. | — 26.43 | | Aug. | + 31.82 |

It is shown graphically in diagram No. 3, how the regularity in the increase of temperature during the summer months contrasts with the irregularity of the winter months. The month of July is the hottest (+ 38°.19), and the month of March the coldest (— 36°.79). In the second winter, December (— 36°.79) was the coldest: range, 73°.1. The mean winter temperature is — 28°.59; for spring, — 10°.59; for summer, + 33°.38; for autumn, — 4°.03; and for the whole year, — 2°.46; this mean temperature is reached in April and October. Comparisons are made between the temperatures of corresponding seasons. Some small corrections to the mean monthly temperatures were introduced for inequality in the length of the months, which gave for the annual mean — 2°.20. These monthly means were then represented

analytically by means of a parabolic curve, with variable parameters, the formula representing that part of the annual variation which lies between March and November generally within a fraction of a degree. The annual variation has likewise been expressed by the usual formula, namely, $T = -2°.20 + 35°.39 \sin(\theta + 251°.43') + 6°.72 \sin(2\theta + 69°.47') = 3°.20 \sin(3\theta + 17°) \pm 1°.6$. The angle $\theta$ reckoning from the first of January. The warmest day is accordingly July 8th, and the coldest, March 1st; the mean temperature of the year is reached April 29th and October 12th.

The influence of the winds on the temperatures is next investigated. By means of the analytical expressions, a table of hourly mean temperatures was constructed, and the same compared with the hourly abstract of observed temperatures, the difference being inserted in the column corresponding to the direction of the wind observed at the same hour. The separate values for the excess or defect in temperature over the mean temperature for each of the eight principal directions of the wind were thus obtained, including also a column for calms. It was found that winds generally tend to elevate, and that calms depress, the temperature. The frequency of calms is greater than that of all the winds combined. The difference in the temperature of the warmest and coldest winds is 2°.8. The bearing of this investigation on an open (or partially open) polar sea can only be fully made out after the construction of a hygrometric and barometric wind-card. The direction of the warmer winds points towards the Spitzbergen sea, and the relative colder winds come in a direction from the northernmost part of continental America. The elevating effect on the temperature of S. E., S., and S. W. gales has generally been referred to. S. E., S., and S. W. (magnetic) winds are from 0°.6 to 1°.4 above, and N. and N. W. (magnetic) winds 1°.4 below the mean temperature; the magnetic variation is 108° west, making the first named directions N. N. E. and E. S. E. (true).

The difference between the observed and mean temperatures of all hours during which snow (or rain) fell were likewise made out for each month of the year. These hours are warmer, on the average, in the winter months by 13° than the corresponding mean temperature, the numbers decreasing regularly from winter to summer, and reversing in June to —1°. On the average during the year, the sensible heat during precipitation was 7°.7 above the mean temperature. In seventeen

months it snowed during 680 hours and rained during 60 hours. Snow fell in all months of the year, but rain only in July.

The returns of maxima of cold during winter were next investigated. These fluctuations of temperature, taking weeks to perform their cycles, attracted Dr. Kane's particular attention, and he seemed to be inclined to consider the phases of the moon as intimately connected with the subject. It was found that the full moon season, with cloudless nights, was in correspondence with the lowest mean temperatures. The march of the mean diurnal temperature (means of six consecutive days each) together with a curve expressive of the number of hours during which snow fell, and another curve representing the amount of transparence of the atmosphere during the same interval, are presented graphically for the two winters. The resulting period for the recurrence of cold was found $= 27^d.7 \pm 1^d.0$; the same was compared with the synodic period of the moon. The mean amplitude of the wave is 19°.8. The analysis of these fluctuations is then entered into, and it was found that a maximum cold is produced by a concurrence of the time of full moon with a perfect calm and a high degree of transparency of the atmosphere, while on the contrary a minimum of cold was produced by the concurrence of the time of new moon with a continued fall of snow, a generally obscured atmosphere, and winds from a direction between N. N. E. and E. S. E. (true), and it is stated as being desirable that the apparent connection of the lunar phases with the transparency and obscurity of the atmosphere should be further investigated. In accordance with Prof. Dove's investigation in reference to a return of cold about May eleventh, it was found that on May 13th (1854), the mean temperature was depressed 9°.3.

A table of hourly corrections for periodic variations was constructed and arranged precisely like similar tables prepared by Prof. Guyot, and published by the Smithsonian Institution. The hours 7, 2, and 9, as adopted by the Institution, give the annual mean, even for this remote station, within 0°.31; the same hours with the last taken twice, give it within 0°.09; the Royal Society hours come within 0°.02; while the hours 6, 2, and 10, and likewise 8 and 8 and 10 and 10, seem to produce the closest approximations.

For the investigation of the solar radiation, hourly readings of the black bulb thermometer, together with numbers expressive of the amount of solar light or transparency of the air have been given for

the months of April to October. This abstract is accompanied by foot notes referring to matters connected with the subject.

The decrease of the temperature corresponding to an elevation of eighty feet above the level of the sea was measured from bi-hourly observations extending over the months of August, September, and October. The quantity varies considerably with each month, and it was found that on the average the elevation corresponding to a change of 1° Fahr. in the mean temperature amounted to 210 feet.

The surface temperature of the sea-water was found to be 28°.7 during the winter months, and constant, and 32°.3 in maximum in the month of July.

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4. ABSTRACT OF THE PRINCIPAL RESULTS OF THE DISCUSSION OF OBSERVATIONS FOR ATMOSPHERIC PRESSURE AT VAN RENSSELAER HARBOR, NORTH GREENLAND, MADE BY THE SECOND GRINNELL EXPEDITION UNDER COMMAND OF DR. E. K. KANE, U. S. N., DURING 1853–'54 AND '55. Presented by Prof. A. D. BACHE, from reduction and discussion by CHARLES A. SCHOTT, Asst. Coast Survey.*

THE barometric observations at Van Rensselaer harbor, North Greenland, were made hourly between June 27, 1853, and January 24, 1855, the record of mean daily values going up to April 22, 1855. The observations here discussed were recorded from September 1, 1853, to January 24, 1855, from a standard mercurial barometer; excepting those before October 23 of the first year, which were taken from an Aneroid whose indications were carefully compared with corresponding ones of the mercurial barometer. It was found that the formula

$$B = A + 0.159^{\text{in.}} + 0.072\,(A - 29.672^{\text{in.}}) + 0.0065\,(T - 75°.8),$$

represented these comparisons within 0.03 inches for the extreme cases. By means of a table these earlier readings were converted into corresponding mercurial barometer readings. The paper next gives a com-

* For the complete paper see Smithsonian Contributions to Knowledge, Vol. XI.

plete record of the hourly observations of pressure and temperature of the daily and hourly means, and the same reduced to 32° Fahrenheit, for each month of the year and years. The average elevation of the instrument above the mean level of the sea was about five feet.

The diurnal variation was found quite perceptible, and a table showing its amount for each month and from all the observations combined has been given, illustrated with a diagram. Owing to the small amplitude, the comparatively short period of observation, and the magnitude of the occasional disturbances, the law of the diurnal variation is apparently subject to considerable fluctuations. The following table gives the mean values.

| Hour. | Means. | Hour. | Means. |
|---|---|---|---|
| 1 | 29.763 | 13 | 29.759 |
| 2 | .765 | 14 | .759 |
| 3 | .766 | 15 | .761 |
| 4 | .766 | 16 | .763 |
| 5 | .766 | 17 | .765 |
| 6 | .766 | 18 | .767 |
| 7 | .766 | 19 | .768 |
| 8 | .762 | 20 | .769 |
| 9 | .762 | 21 | .770 |
| 10 | .764 | 22 | .771 |
| 11 | .764 | 23 | .769 |
| Noon. | .763 | Midn't. | 29.768 |
| | | Means, | 29.765 |

It has been expressed by the formula

$$B = 29.765^{\text{in.}} + 0.0034^{\text{in.}} \sin(\theta + 290°) + 0.0022^{\text{in.}} \sin(2\theta + 204°),$$

the angle $\theta$ counting from noon and at a rate of 15° an hour. The principal feature of the curve (Diagram No. 1) is the afternoon inflection with a maximum pressure at about 10 P. M. The principal minimum is reached at 1 P. M. The 10 P. M. maximum is in strict conformity with the general law in the northern hemisphere, but the 1 P. M. minimum seems to be about three hours earlier than its time of occurrence at more southern stations. The maximum difference is 0.0097 inches. All the results stated here refer to the gross pressure of the atmosphere. A long series of hygrometric observations were made,

but owing to the peculiar delicacy which such observations require in a latitude of such low temperature, these observations were found not sufficiently reliable in their results, and have consequently not been taken into account. The annual fluctuation of the atmospheric pressure has been made out, combining results for the same months in different years, and it was found that the barometer is above the mean in the months of January, February, March, April, and May, and below the mean in the remaining summer and autumn months, as exhibited in the following table.

| Year. | Jan'y. | Feb'y. | Ma'ch. | April. | May. | June. | July. | Aug. | Sep'. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1853 | | | | | | | | | 29.662 | 29.815 | 29.713 | 29.821 |
| 1854 | 29.458 | 29.654 | 29.798 | 29.977 | 29.937 | 29.714 | 29.736 | 29.689 | 29.645 | 29.686 | 29.792 | 29.675 |
| 1855 | 30.089 | 20 032 | 29.693 | 29.820 | | | | | | | | |
| Means, | 29 772 | 29.843 | 29.745 | 29.898 | 29.937 | 29.714 | 29.736 | 29.689 | 29.653 | 29.750 | 29.753 | 29.748 |

The maximum pressure was observed in May, the minimum in September; the range between these two months was 0.284 inches. The following expression shows the general course of the annual variation:

$$B = \overset{\text{in.}}{29.770} + 0.079 \sin(\theta + 4°) + \overset{\text{in.}}{0.044} \sin(2\theta + 194°),$$

the angle $\theta$ counting from January 1, and expressed at a rate of 30° a month. The result by the formula is given graphically. (Diagram No. 2.)

The computed annual range is 0.207 inches. If we add 0.005 inches as correction to the constant 29.770 to refer the result to the level of the sea, we obtain for the observed mean barometric height at that level in north latitude 78° 37′, 29.775 English inches. It may be remarked that the Van Rensselaer station is about 14° higher north than the latitude on which the mean reading of the barometer at the level of the sea is a minimum.

The irregular oscillations of the pressure daily, monthly, and annually, were considered next. The irregular changes in the atmospheric pressure are like those of the temperatures, much greater in winter than in summer. If we deduce the average difference, irrespective of sign,

in the barometric height between any two consecutive days, we obtain a regular progression in the mean monthly values, namely:

| | in | | in. |
|---|---|---|---|
| January, | 0.17 | July, | 0.09 |
| February, | 0.26 | August, | 0.10 |
| March, | 0.17 | September, | 0.11 |
| April, | 0.12 | October, | 0.15 |
| May, | 0.14 | November, | 0.17 |
| June, | 0.10 | December. | 0.26 |

The average for the whole year is 0.15 inches; in the months of December, January, and February the variability between successive diurnal means is a maximum (0.23 inches), and in the months of June, July, and August it reaches a minimum value (0.10 inches).

The following table contains the maxima and minima pressure as observed in each month, also the extreme ranges.

| | MAXIMA. | | | MINIMA. | | | RANGE. |
|---|---|---|---|---|---|---|---|
| Month. | 1853. | 1854. | 1855. | 1853. | 1854. | 1855. | Inches. |
| September, | 30.06 | 30.25 | | 29.07 | 29.01 | | 1.11 |
| October, | 30.50 | 30.16 | | 29.11 | 29.00 | | 1.28 |
| November, | 30.26 | 30.41 | | 29.05 | 29.01 | | 1.30 |
| December, | 30.50 | 30.37 | | 29.01 | 28.89 | | 1.48 |
| January, | | 29.92 | 30.97 | | 28.86 | 29.30 | 1.36 |
| February, | | 30.45 | | | 28.84 | | 1.61 |
| March, | | 30.49 | | | 29.18 | | 1.31 |
| April, | | 30.37 | | | 29.28 | | 1.09 |
| May, | | 30.49 | | | 29.19 | | 1.30 |
| June, | | 30.19 | | | 29.41 | | 0.78 |
| July, | | 29.97 | | | 29.40 | | 0.57 |
| August. | | 30.05 | | | 29.22 | | 0.83 |

This mean range of monthly extremes likewise exhibits a regular progression; the mean range is 1.17 inches; the maxima rise on the average to 30.31 inches, and the minima fall to 29.14 inches. As in the preceding variation the greatest monthly range takes place in December, January, and February (1.48 inches), and the least in June, July, and August (0.73 inches). The absolute highest reading was 30.97 inches, recorded for the morning of January 21, 1855; the abso-

lute lowest reading was 28°.84, taken near noon of February 19, 1854; and the extreme range observed was 2.13 inches. Of the gales noted in my discussion of the winds at Van Rensselaer harbor, only a few were attended by a noticeable amount of change in atmospheric pressure. Before the setting in of the gale of December 29, 1853, the barometer fell 0.35 inches in nine hours, while before that of December 18, 1854, it fell 0.36 inches in five hours.

The state cf the barometer during hours of the fall of snow or of rain was next investigated, and it was found from the comparison of recorded hours (563), that there is a difference of 0.006 inches, by which quantity the barometer is lower during snow than during clear weather.

The connection of the atmospheric weight with the direction of the wind requires, in order to find its average effect, a great number of observations, particularly on account of the irregular oscillations of the barometer in the winter months. The following results are derived from a comparison of the barometric readings of the hours 6 A. M., noon, 6 P. M., and midnight for each day with the respective mean monthly reading during seventeen months of observations. These differences were then arranged according to the directions of the wind; and the results are as follow: $\pm$ indicating $\left\{\begin{matrix}\text{above}\\\text{below}\end{matrix}\right\}$ the mean.

| Magnetic Direction. | in. | |
|---|---|---|
| N. | — 0.022 | |
| N. E. | + 0.072 | On account of the comparatively small number of observations for these directions, they may be combined E. N. E. — 0.044 inches. |
| E. | — 0.100 | |
| S. E. | 0.000 | |
| S. | + 0.038 | From 1050 comparisons of calms and barometric readings, the latter were found 0.005 inches above their mean value. |
| S. W. | + 0 045 | |
| W. | — 0.031 | |
| N. W. | — 0.031 | |

It is only during S. and S. W. (magnetic) winds that the barometer rises above the mean value; during all other winds it is depressed.

To show the dependence and relation of the atmospheric pressure to each wind and to the temperature of the same, it was thought best to put the relation of the atmospheric pressure as well as that of the temperature, to the winds, in an analytical form. In the discussion of the observed temperatures at Van Rensselaer harbor a table was given showing the dependence of the temperature on the direction of the

wind, and if we deduct the mean elevation of temperature by the winds from each separately, we obtain the following table of the effect of each wind on the temperature, $\pm$ indicating an $\left\{\begin{matrix}\text{elevation}\\\text{depression}\end{matrix}\right\}$ of temperature.

| Magnetic Direction. | ° | Magnetic Direction. | ° |
|---|---|---|---|
| N. | −1.4 | S. | +0.6 |
| N. E. | 0.0 | S. W. | +0.4 |
| E. | −0.1 | W. | +0.1 |
| S. E. | +0.9 | N. W. | −1.4 |

Counting $\theta$ from the north, or as belonging to a north wind (magnetic) round by E. to 360°, we obtain, by using Bessel's formula,

$$T = +1°.02 \sin(\theta + 286°),$$

or for the *true* directions,

$$T = +1°.02 \sin(\theta + 34°),$$

and similarly from the barometric relation to the winds,

$$B = +0°.018 \sin(\theta + 354°).$$

A comparison of the angular constants in these two expressions does not show a correspondence of the wind of maximum temperature with a minimum pressure; on the contrary, there is a much nearer correspondence of the wind of maximum temperature with maximum pressure. According to the first formula, the hottest wind is from the direction N. E. $\frac{1}{2}$ E. (true) and by the second formula the maximum atmospheric pressure is from the direction E. (true) nearly.

5. ABSTRACT OF THE PRINCIPAL RESULTS OF THE DISCUSSION OF THE OBSERVATIONS FOR DIRECTION AND FORCE OF THE WIND AT VAN RENSSELAER HARBOR, NORTH GREENLAND, SECOND GRINNELL EXPEDITION, UNDER THE COMMAND OF DR. E. K. KANE, U. S. N., IN 1853, 1854, AND 1855. Presented by Prof. A. D. BACHE, from reduction and discussion by CHAS. A. SCHOTT, Assistant Coast Survey.*

THE observations of the direction and force of the wind at Van Rensselaer harbor extend over the same period as the other meteorological observations, namely, from September 1, 1853, to May 1, 1855. With few exceptions, these observations were made hourly, and their record available for discussion extends to January 24, 1855. Those taken after this date will be found recorded, in the second volume of the narrative.

No self-registering anemometer was used, the force being estimated, and the direction of the wind given, by compass, and noted in reference to eight principal directions. The course of the wind thus given refers to the lower or surface stratum of the atmosphere. The hourly record contains the force of the wind estimated on a scale from 0 (calm) to 10 (hurricane), which numbers were afterwards converted into velocities, in miles per hour, according to Smeaton's table. In this abstract the fall of snow and rain is likewise noted. The discussion considers the resulting direction and force of the wind as well as the quantity of the air passing over the place of observation. In regard to the resulting direction and velocity of the wind for any given period, the usual formula of Lambert has been so far modified as to include velocity without dependence on the relative frequency of the winds alone. The quantity of air passed over the place of observation by winds from the southward and by winds from the westward has been expressed by the formula,

$$R_s = s_1 \cos \theta_1 + s_2 \cos \theta_2 + s_3 \cos \theta_3 + \ldots\ldots$$
$$R_w = s_1 \sin \theta_1 + s_2 \sin \theta_2 + s_3 \sin \theta_3 + \ldots\ldots$$

from which expressions we deduce the resulting direction and velocity.

* For the complete paper, see Smithsonian Contributions to Knowledge, Vol. XI.

The angle $\theta$ counts from the south round by west, and $s$ is the length of the path (in miles) described by a direction. The formulæ were put in a convenient shape for the observations on hand, and their development supposes that all particles of the air surrounding the station equally participate in the general motion or that all particles describe equal and parallel paths. The method is illustrated by a complete example for the observations during September, 1853.

The resulting monthly direction $\varphi$ of the wind is obtained by adding up the velocity numbers for each wind separately from the hourly observations, and then treating the results according to the formulæ given above. The sum of the velocity numbers for each wind as well as the resulting direction of the wind for each month was made out. The magnetic direction is converted into true direction by applying the magnetic variation, namely, 108° 12′ west.

| | $R_s$. | $R_w$. | Mag. $\phi$. | True $\phi$. | | $R_s$. | $R_w$. | Mag. $\phi$. | True $\phi$. |
|---|---|---|---|---|---|---|---|---|---|
| | | | ° | ° | | | | ° | ° |
| Sept. | +1059 | +904 | 40 | 292 | March, | + 541 | — 30 | 357 | 249 |
| Oct. | 1682 | 486 | 17 | 269 | April, | +1408 | + 670 | 25 | 277 |
| Nov. | 901 | + 89 | 6 | 258 | May, | +1014 | 512 | 27 | 279 |
| Dec. | 1530 | —482 | 342 | 234 | June, | — 937 | 1027 | 132 | 24 |
| Jan. | 659 | +174 | 15 | 267 | July, | + 745 | + 805 | 47 | 299 |
| Feb. | +1800 | —288 | 351 | 243 | Aug. | + 783 | — 8 | 359 | 251 |

By means of $\Sigma R_s$ and $\Sigma R_w$ for the respective periods the following resulting directions of the wind for each season were obtained.

| | Mag. $\phi$. | True $\phi$. | True dir. |
|---|---|---|---|
| Winter (Dec. Jan. Feb.), | 351 | 243 | E. 27 N. |
| Spring (March, April, May), | 21 | 273 | E. 3 S. |
| Summer (June, July, Aug.), | 72 | 324 | E. 54 S. |
| Autumn (Sept. Oct. Nov.), | 22 | 274 | E. 4 S. |
| Year, | 19 | 271 | E. 1 S. |

From the above results we see that in general the mean true direction of the wind is from the eastward, varying in the several months to

the northward and southward of it. There is but one exceptional month (June) in which the wind veers round to the westward of south. In spring and autumn the mean true direction is almost exactly east, as well as for the whole year. In winter it is E. N. E. and in summer S. E. by S.

The average velocity in the resulting direction of the wind, which is necessarily smaller than the mean velocity of the several winds, on account of the partial neutralization of opposing winds, is found by dividing the quantity R by the actual number of observations. This quantity was made out for each month, and the average value for the whole year is 4.5 miles per hour. The average velocity with which each of the eight winds passes over the place of observation in each month and for the whole year, is found by dividing the sum of the velocity numbers in the period by their respective number of entries in the table. These results are shown in the annexed table.

| Mag. dir. | Jan. | Feb. | Mar. | Apr. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | mls. | mls. | mls. | mls. | mls. | mls. | mls. | mls. | mls. | mls. | mls. | mls. |
| S. | 2.9 | 9.4 | 4.3 | 3.3 | 10.9 | | 18.1 | 12.1 | 10.1 | 8.2 | 6.3 | 9.1 |
| S. W. | 8.2 | 11.3 | 1.5 | 14.6 | 7.1 | 8.7 | 23.1 | 1.6 | 14.2 | 9.2 | 15.6 | 11.5 |
| W. | 3.2 | 2.1 | 2.0 | 1.0 | 1.0 | 4.3 | 2.9 | 6.3 | 5.6 | 10.4 | 1.5 | 7.0 |
| N. W. | 2.7 | 1.5 | 6.4 | 1.5 | 1.6 | 6.8 | 4.1 | 7.2 | 9.7 | 10.4 | 4.2 | 3.6 |
| N. | 1.7 | 2.3 | 1.0 | 2.3 | 2.2 | 11.4 | 1.7 | 3.6 | 8.4 | 3.3 | 2.6 | 2.0 |
| N. E. | 1.6 | 1.0 | 1.0 | | | 22.4 | 4.5 | 3.0 | 2.0 | 4.5 | 4.8 | 7.2 |
| E. | 1.0 | 8.5 | 11.5 | 1.0 | 1.5 | | 2.5 | 2.8 | 5.1 | 2.1 | 3.5 | 13.1 |
| S. E. | 4.3 | 6.7 | 3.3 | 4.1 | 6.9 | | 2.9 | 8.8 | 5.7 | 5.7 | 5.8 | 11.1 |
| Weighted mean. | 4.4 | 7.9 | 3.8 | 5.4 | 4.7 | 7.6 | 8.4 | 8.0 | 9.6 | 7.6 | 6.9 | 9.5 |

Accordingly the winds blow with their greatest strength in the months of September and December, and with their least strength in the months of March and May; in the former the mean velocity is 9.6, and in the latter case 4.2 miles per hour. In the following table is exhibited the mean velocity of each wind separately, and the results are illustrated by diagram No. 1.

| Mag. dir. | Mean velocity in miles per hour. | Mag. dir. | Mean velocity in miles per hour. |
|---|---|---|---|
| S. | 7.4 | N. | 3.5 |
| S. W. | 11.2 | N. E. | 6.2 |
| W. | 4.1 | E. | 6.0 |
| N. W. | 4.8 | S. E. | 6.3 |

Of all winds the S. W. (mag.) blows with the greatest force, and the north wind with the least.

The sum of the velocity numbers, or the number of miles travelled over by the air in any direction for any given period, may be called the quantity of wind which has been transferred over the place of observation during that time. The variations in the density of the air have not been considered in this connection. The quantity of wind and direction for each month, as well as the number of hours during which it blows have been tabulated, and the following are the sums for each wind during the year.

| Mag. dir. | Quantity in miles. | Hours of occurrence | |
|---|---|---|---|
| S. | 6119 | 821 | |
| S. W. | 8004 | 708 | |
| W. | 962 | 233 | |
| N. W. | 3225 | 660 | Number of hours of calms, 5063. |
| N. | 1000 | 287 | |
| N. E. | 337 | 53 | |
| E. | 672 | 113 | |
| S. E. | 5200 | 822 | |

The tabular results are laid down graphically in rectangular and circular diagrams (Nos. 2 and 3), which also give the relative frequency of the winds. The greatest quantity of air moved over the place during the year comes from a quarter bounded by the true directions S. E. by E. and N. N. E. The prevailing wind also comes from this quarter. With the exception of the true S. W. by S. winds, there is comparatively hardly any wind from the remaining directions; the quadrant between true west and north being particularly deficient in this respect. The calms greatly predominate, there being, during the year, more

hours of calm (5063) than hours of wind (3697) from any quarter, a circumstance quite characteristic of the locality.

The number of consecutive hours during which any one wind blew on the average has also been tabulated, giving 2.1 hours for the east (mag.) wind and 3.4 hours for S. W. and N. W. winds. Calms last on the average, 7.8 hours.

Owing to the great number of calms, and partly also to the generally small quantity and velocity of the winds between the true south and true north-west directions, the law of rotation cannot well be verified in this locality. There is, however, a tendency of the (mag.) S. S. E. and E. winds to shift in accordance with the law, in the proportion of three cases to one case against it.

Between September 1, 1853 and January 24, 1855, there were recorded thirteen gales, not confined to any particular season. They lasted about seven hours, and in summer with a tendency to blow from E. S. E. (true) and in winter from N. N. E. (true). These two directions are the only ones from which gales were experienced.

The investigation of the connection of the temperature and weight of the atmosphere with the direction of the wind will be found noticed in the paper on the temperature and barometic pressure.

The dependence of the fall of snow or rain on the direction of the wind is shown in the following table, which contains the aggregate number of hours during which snow (or rain) fell for any direction of the wind, and during calms.

| Dir. (mag.) | Hours. | Dir. (mag.) | Hours. | |
|---|---|---|---|---|
| S. | 52 | N. | 45 | |
| S. W. | 71 | N. E. | 6 | Calm, 328 hours. |
| W. | 43 | E. | 10 | |
| N. W. | 110 | S. E. | 75 | |

The snowy or rainy quarter is between N. N. E. (true) and E. S. E. (true), or from the direction of the Spitzbergen sea, and also from the direction S. S. W. (true), or from the Upper Baffin bay. During almost half the time it snowed during calms. From the directions W. N. W. (true) and N. N. W. (true) there was hardly any precipitation.

## 6. On the European Storm of December 25, 1836. By Prof. Elias Loomis, of New York.

About the 25th of Dec., 1836, a very violent storm of rain and snow was experienced over a large portion of Europe. In England there occurred one of the severest snow-storms ever known in that country. The snow commenced falling on the 24th, and continued without intermission through the 25th and until the evening of the 26th. The depth of snow was such as to render the roads impassable, the drifts rising in some places to the height of ten or twelve feet. The editor of the London Times remarks: "Never before within our recollection was the London mail stopped for a whole night at a few miles from London, and never before have we seen the intercourse between the southern shires of England and the metropolis interrupted for two whole days." The area covered by the rain and snow formed nearly a circle of about 1,500 miles in diameter. North of Switzerland the moisture was precipitated in the form of snow; south of Switzerland it was precipitated in the form of rain. Throughout Italy the fall of rain was heavy, and at Florence it exceeded three inches.

Over nearly the whole of this area of rain and snow the barometer sunk below its mean height. At the centre of this area the barometer was depressed one inch *below* its mean height, while at Iceland the barometer stood more than an inch *above* its mean height. The point of greatest barometric depression was situated near Switzerland, and this may be regarded as the centre of the storm. For more than four days this centre of the storm was nearly stationary, oscillating alternately eastward and westward through a range not exceeding 250 miles. Throughout the entire area of rain and snow, the winds for several days exhibited a tendency to crowd inward towards the centre of the storm; not in the direction of radii, but spirally inward, circulating round the centre in a direction contrary to the motion of the hands of a watch. This tendency was very strongly indicated from the 24th to the 28th of December.

From a comparison of the European storm of December 25, 1836, with the American storm of December 20, 1836, and also the storms of February 4 and 16, 1842, Prof. Loomis has been led to the following generalizations. Some of these conclusions are substantially the

same as given by Mr. Espy in his Fourth Meteorological Report, but several of Mr. Espy's conclusions are only true when applied to American storms:

1. The area covered by a violent storm of rain or snow is sometimes nearly circular in form; sometimes its form is very much elongated or elliptical, its length being two or three times its breadth; and frequently its form is very irregular. In the winter storms of the United States, the north and south diameter is generally very much greater than the east and west diameter.

2. When storms are circular, the area of rain or snow is sometimes 1,500 miles in diameter; when their form is elliptical, the area of rain or snow is sometimes 1,000 miles wide, and 2,000 or 3,000 miles long.

3. Violent storms sometimes remain sensibly stationary for four or five days, but generally the centre of a storm has a progressive movement along the earth's surface. The rate of this progress has been observed to vary from zero to forty-four miles per hour. From our limited number of observations, it seems probable that American storms travel more rapidly than European storms.

4. Within the limits of prevalent westerly winds, when violent storms advance with considerable rapidity, the direction of progress is always from west to east. This direction is not absolutely uniform, but has been observed to vary from about due east, to north fifty-four degrees east.

5. Great storms of rain and snow are accompanied by a depression of the barometer near the centre of the storm, and a rise of the barometer near the margin; but this rise is not generally uniform along the entire margin.

6. The depression of the barometer at the centre of a storm sometimes amounts to more than an inch *below* the mean height; and the rise along some portion of the margin sometimes amounts to more than an inch *above* the mean height.

7. Winter storms commence gradually, and generally attain their greatest violence only after a lapse of several days; after a time their violence gradually diminishes, and at length they disappear entirely. This succession of changes requires a period of several days, sometimes one or two weeks, and possibly even longer. Sometimes all these changes are experienced over the same country; that is, the storm makes no progress from place to place. More commonly, how-

ever, the storm travels along the earth's surface; and although the same storm may continue for one or two weeks, or even longer, its duration at any one place may not exceed one or two days.

8. For several hundred miles on each side of the centre of a violent storm, the wind inclines inward towards the area of least pressure, and at the same time circulates around the centre in a direction contrary to the motion of the hands of a watch.

9. In Europe as well as in the United States, on the north side of a great storm, the prevalent winds are from the north-east; while on the south side they are from the south-west.

10. The force of the wind is proportioned to the magnitude and suddenness of the depression of the barometer; but very near the centre of a violent storm there is often a calm.

11. On the borders of the storm, near the line of maximum pressure, the wind has but little force and tends outward from the line of greatest pressure.

12. The wind uniformly tends from an area of high barometer towards an area of low barometer; and this is probably the most important law regulating the movement of the wind.

13. In a great storm, the centre of the area of high thermometer frequently does not coincide with that of the area of low barometer, or with the centre of the area of rain and snow. In the United States, on the north-east side of a storm, at a distance of over 500 miles from the area of rain and snow, the thermometer sometimes rises even twenty degrees above its mean height.

14. The storms of Europe are very much modified, and sometimes in a great measure controlled by the Alps of Switzerland. By the interposition of these mountains, the air which sweeps over them is forced up to a great height where it is suddenly cooled; its vapor is condensed; heat is accordingly liberated by which the surrounding air is expanded, and rises above the usual limit of the atmosphere. It thence flows off laterally, leaving a diminished pressure beneath the cloud; that is, the barometer shows a diminished pressure in the neighborhood of the mountain. The mountain thus becomes the centre of a great storm, and the storm may continue stationary for several days, being apparently held in its place by the action of the mountain.

## 7. On the Winds of the Southern Hemisphere.* By Prof. J. H. Coffin, of Easton, Pa.

The preparation of this paper is due to the liberality of the Smithsonian Institution, which has not only allowed me the free use of all the material at its command, pertaining to the subject of winds in all parts of the globe, but also has generously defrayed a considerable part of the expense of making the necessary calculations.

The results I am about to present are confined chiefly to the summer and winter months in the southern hemisphere. Those for the spring and autumn, and also the general results for the year have been computed, but in order to illustrate them properly, separate diagrams† would be needed. I have only added them in a few instances for parts of the ocean where observations for the other seasons of the year were wanting, distinguishing them by the color of the arrows by which they are represented; namely, green for spring, and yellow for autumn. Indeed, I offer this paper, not as a complete discussion of the subject of which it treats, but such only as my time and other duties have allowed me to prepare, reserving a more full one for some future occasion. For the sake of brevity and to avoid confusion when I come to discuss the winds of both hemispheres together, as I am preparing to do, the groups of months constituting the different seasons are designated by the same name in the southern as in the northern hemisphere, without reference to the fact that as it regards physical phenomena the seasons in the two are opposite.

Meteorological observations have been taken on land only at comparatively few stations in the southern hemisphere, and most of my mate-

* Just as I was on the eve of starting to attend this meeting I received, through the kindness of Capt. Maury, in advance of publication, some sheets from the forthcoming enlarged edition of his Physical Geography of the Sea, containing a new chapter, which he has introduced, on the winds of the southern hemisphere,—and on examining them since my arrival here, I find that on many points his results coincide so nearly with those given in this paper, that I have hesitated about offering it. Yet as our modes of investigation are unlike, and as his field of research does not extend far enough south to embrace any parts of the system of southern polar winds, I conclude to present it as it is.

† The diagrams accompanying this paper are not received.—J. L., Editor.

rial is derived from the published wind charts of Capt. Maury, which now cover all those portions of the southern oceans lying between the equator and the sixtieth parallel of latitude, and from the one hundred and sixty-fifth meridian of west longitude from Greenwich, eastward to the one hundredth meridian of east longitude, — or about three fourths of the way round the globe. For more southern latitudes I have had to resort to the published reports of Cook, Ross, Wilkes, and other antarctic explorers.

The aggregate amount of time embraced is about 240 years, or 318 summers and 644 winters; so that my results embody the experience of a man, if such could be, who had spent all his winters and half of his summers in these southern seas for nearly 650 years. All these summers except nine days, and 641 of the winters were spent north of 60°, and except in the vicinity of Cape Horn, nearly all the former were spent north of 45°, and so afford, of course, only very unsatisfactory data for that part of the year. Between the parallels 60° and 65° I have observations for nine days in summer and 194 in winter, and south of the latter parallel for 168 days in winter and eleven in spring. I have just been informed that a new whaling ground has been discovered in lat. 60° to 65°, south of Kerguelen Land, and if so the log-books of vessels that visit it will afford the means of filling an important blank, and I shall endeavor to procure extracts from them. The following tabular statement will show more fully the geographical distribution of the observations north of lat. 60°.

| | PACIFIC OCEAN. | | ATLANTIC OCEAN. | | INDIAN OCEAN. | | TOTAL. | |
|---|---|---|---|---|---|---|---|---|
| | No. of months in summer. | No. of months in winter. | No. of months in summer. | No. of months in winter. | No. of months in summer. | No. of months in winter. | No. of months in summer. | No. of months in winter. |
| Lat. 0° to 40° south, | 180 | 431 | 329 | 451 | 369 | 539 | 878 | 1421 |
| " 40 to 45 " | 14 | 82 | 7 | 54 | 7 | 104 | 28 | 240 |
| " 45 to 60 " | 34 | 178 | 13 | 60 | 0 | 22 | 47 | 260 |
| Total, | 228 | 691 | 349 | 565 | 376 | 665 | 953 | 1921 |

The method of investigation has been the same as I have heretofore described, namely, calculating, after the ordinary method of composition of forces, the mean resultant of all the winds, both as

to direction and amount. It may be illustrated by supposing a balloon to be set loose, and to float freely in the air over any district of country, or portion of the ocean, and it is required to find its distance and direction from its starting-place after any given time, in any given season of the year. The method by Lambert's formula is the same in principle, but is applicable without modification only where the direction of the wind is noted but for eight points of the compass.

Most of the calculations have been made by Mr. Benjamin F. Stem and Mr. James T. Doran, both former graduates of La Fayette College, and both careful and accurate mathematical computers.

The results I have delineated upon maps of the hemisphere by means of arrows representing by their direction the mean resultant direction of the winds, and by their length, exclusive of the point, the ratio that the progressive motion of the wind bears to the total motion, the unit being one inch. Thus, for example, if the wind progressed in its mean direction fifty miles while travelling by its circuitous route one hundred, it would be represented by an arrow half an inch long, — and so for any other ratio. Resultants deduced from less than 150 days' observation are marked by a note of interrogation, to show that they are considered doubtful, — those deduced from less than fifty, by two such marks, and those from shorter periods with three or four according to the degree of doubt.

With these preliminary remarks, I proceed now to give a brief outline of the results to which I have arrived, referring those interested to the diagrams for a graphical illustration of them.

If I have shown satisfactorily heretofore that three great systems of wind exist in the northern hemisphere, we should expect corresponding ones in the southern, and accordingly I find unmistakable indications that such is the case; namely, 1st, a system of easterly and south-easterly winds near the equator, — 2d, a system of westerly and north-westerly winds in the middle latitudes, — and 3d, a system of south-easterly winds near the south pole. I will speak of the features of each of these systems separately.

The zone of the equatorial or trade wind system appears to be somewhat narrower than in the northern hemisphere, extending from the equator only to about the thirtieth parallel of latitude in summer, and to about the thirty-third in winter. Near the equator the progressive

motion is relatively very great, generally over ninety per cent. of the total motion; but as we approach the southern limit of the zone it dwindles down to ten or fifteen per cent., and often less. The first part of this statement is true more especially over the Atlantic and Pacific oceans, for over the Indian ocean the system is not developed in winter further north than latitude 10°. While the winds of this system are generally south-easterly, yet owing to local disturbances of greater or less extent, they are by no means uniformly so. If we start from the 180th meridian from Greenwich, and travel eastwardly, we find the winds at first nearly due east, perhaps a little to the north of east, particularly in the winter months. But as we advance they grow more and more southerly till we reach the west coast of South America, where the direction coincides very nearly with that of the coast line. Crossing that continent we find the system nowhere better developed than over the middle portions of the Atlantic, but as the current approaches the coast of Brazil, it seems to be split asunder, near Cape St. Roque, one branch following the north-eastern and the other the south-eastern coast. Near the west coast of Africa the phenomena resemble those which I have just described on the corresponding coast of South America. Through the entire width of the African continent we have no observations except a few imperfect ones by Livingstone. In the Indian ocean the system is well developed in the summer season, but disappears entirely in winter north of lat. 10°, and is replaced, for the most part, by a current nearly in the opposite direction. To understand and exhibit clearly the peculiarities of the winds in this disturbed region, we need to extend it north of the equator, so as to connect them with those of the Arabian Sea and Bay of Bengal. Along the parallel of 10° there is in winter but little progressive motion in any direction, but soon after passing south of it we fall in with the regular south-east "trades," which continue till they break up at the southern limit of the system, about lat. 33°, as already remarked. A singular feature is noticeable in the winter winds near this limit, in longitude 55° to 65° east, a thousand miles or so south-east from Madagascar. There is a perfect eddy or whirlpool in the air, several hundred miles in diameter, in the middle of which there is no progressive motion whatever. In the summer season this feature is less marked, if it exists at all. There are one or two other places in the Atlantic and Pacific oceans where there seems to be a tendency to a movement of the same kind, but nowhere else is it so clearly defined.

The system of westerly and north-westerly winds comprises a zone bounded on the north by that of the equatorial winds, just described, and extends southerly in the winter season to latitudes varying from about 61° in the southern Indian ocean to about 65° or 66° in the south Pacific. Its southern limit in the summer season (their winter), which I have ventured to draw in lat. 50°, is as yet entirely a matter of conjecture, no observations having been obtained from which to determine it. Indeed the whole southern ocean, south of lat. 45°, except in the vicinity of Cape Horn, appears to be unfrequented by ships in that season of the year, not a solitary observation being given by Capt. Maury through the entire width of the Atlantic and Indian oceans, and but few in the Pacific. Our knowledge, therefore, of the summer winds of this zone is confined chiefly to its northern half, from lat 30° to 45°. The winds of this system, both in summer and winter, are remarkable for the uniformity of their mean direction, which, contrary to what has been commonly supposed, is far more uniform than it is in the zone of the trade winds, though the relative progressive motion is considerably less, rarely reaching sixty per cent. Of all the reliable resultants delineated in the well-defined portions of the system, amounting in the whole to over 250, every one in winter, and all but seven in summer, are westerly, and most of them are within a few degrees of the general mean, which is about N. 75° to 80° W. Of the seven exceptions, five are contiguous to the coasts of Africa, South America, and New Zealand, where there are doubtless local influences; and the general uniformity in the mean direction results no doubt from there being no large bodies of land in the track of the current, except the southern peninsula of South America.

Of the third system, comprising the southern polar winds, our knowledge is confined to the months of winter and early spring, no ship, so far as I can learn, having ever visited this inhospitable region in the other parts of the year. The aggregate amount of time for which I have obtained observations within this zone is but 271 days, 92 of which were spent along the margin, and 179 at ten different places south of lat. 65°, where for the most part, the system may be supposed to be fully developed. From the analogy of the northern hemisphere we should expect the winds of this system to be easterly and southerly, and accordingly I find that while the resultants at these ten places show great discrepancies, as appears from the following tabular statement, and as should be expected from the fewness of the observations from

which they were computed, yet every one of them is easterly, which is the more remarkable, and shows the contrast the more clearly, since in the contiguous system, as I have just remarked, every one during the same season of the year was westerly.

MEAN DIRECTION OF SOUTHERN POLAR WINDS.

| Latitude (south). | Longitude from Greenwich. | Season of the year. | Mean direction. | No. of days. | Observers. |
|---|---|---|---|---|---|
| 60° to 65° | 5° to 50° W. | Winter, | S. 33° 19′ E. | 67 | Ross. |
| " " " | 11 to 14 " | Spring, | S. 11 3 W. | 2 | " |
| " " " | 12 to 14 E. | Winter, | S. 22 30 E. | 2 | Cook. |
| " " " | 28 to 47 " | do. | S. 17 13 W. | 8 | " |
| " " " | 95 to 115 " | do. | S. 9 53 W. | 13 | Ross & Wilkes. |
| 65° to 70° | 135° to 150° W. | Winter, | N. 15° 6′ E. | 5 | Cook. |
| " " " | 100 to 110 " | do. | N. 72 4 E. | 9 | " |
| " " " | 8 to 20 " | Spring, | S. 57 47 E. | 7 | Ross. |
| " " " | 39° 35′ E. | Winter, | S. 78 45 E. | 1 | Cook. |
| " " " | 105° to 160° E. | do. | S. 5 45 E. | 22 | Wilkes. |
| " " " | 166 to 176 " | do. | S. 5 37 E. | 60 | Ross. |
| 70 to 75 | 106 to 108 W. | do. | N. 45 0 E. | 2 | |
| " " " | 15 to 18 " | Spring, | N. 55 48 E. | 4 | Ross. |
| " " " | 166 to 176 E. | Winter, | S. 38 42 E. | 35 | " |
| 75 to 80 | 168 W. to 166 E. | do. | N. 88 41 E. | 34 | " |

Of the five resultants near the margin of the zone all are from nearly a south point, — two of them a little east, and three of them a little west, — and of the whole fifteen within the zone only five tend towards the pole, and two of them but slightly so, while ten recede from it, generally at a large angle. Moreover, these five are all easterly, so that in the entire zone there is not a solitary resultant from the N. W. quarter, — the very quarter from which most of the winds in the contiguous zone come. It is true that the length of time covered by the observations is too short for us to place full confidence in the results, but is it not singular that the experience of so many different navigators, in different parts of the sea, should so nearly coincide, if it is all the result of mere accident, and not of any general law? There is one other consideration, however, that I ought in candor to allude to, and which, I confess, greatly impairs my confidence in results obtained in high latitudes both northern and southern. I refer to the great declination of the magnetic needle, and the want of care, often, on the part of navigators to distinguish, in their records, between the magnetic and astronomical meridians.

In what I have said thus far there has been no allusion to any theory to account for the facts I have described, and it was not my intention to have touched upon that subject at all. But since it has been asserted here that the whole fabric is mere theory, — the result of the Hadley "disease," — perhaps the association will indulge me in a few remarks on that head. And first let me say that very few of the calculations, that furnished the results I have exhibited, were made by myself. Over ninety-nine per cent. of them were made by persons whom I employed for the purpose, and who, though skilled in resolving a traverse, or in the composition of forces, knew nothing about winds, nor the bearing of the results they were bringing out, and therefore could not be warped by theory. They merely took the data, made the calculations, page after page, and returned their work to me. Facts thus determined show beyond the possibility of doubt, that the air at the surface of the earth approaches the equator, and the parallels of 60°, or thereabouts, on both sides. Now what becomes of it when it gets there? Is it annihilated? or is there a funnel on some parts of the globe, where it blows back to the regions from whence it came? The former is certainly inadmissible, and evidence has been sought in vain for the latter. It must then rise, notwithstanding the gentleman's * merriment at the idea, and whether we can account for it or not, — and if he can show that his magnetism or electricity is a probable cause of such a movement, we certainly shall not refuse to accept it. Again, there is abundant proof that the surface wind blows from the poles, and from the parallels of 30°. Whence is it supplied? There is surely no new creation of air for the purpose, — it must descend from above.

In seeking for the true cause of these movements, while the heat of the sun is undoubtedly the primary one, there are others which Hadley and those who have succeeded him, have overlooked. Such is the one that Prof. Ferrel has discussed with so much ability in the numbers of the Mathematical Monthly. If there were no other disturbing force it is obvious that the entire atmosphere must assume a spheroidal form, more or less oblate according to the rate of its diurnal easterly motion. If this motion were increased it would flow toward the equator, and if diminished it would fall back toward the poles. And what would be true of the entire atmosphere would be true in like

* Judge Butler of Connecticut.

manner of any portion of it. Consequently if the diurnal motion of the air, at any place, is greater than that of the earth, it must tend toward the equator,—if less, toward the poles. Now the air which rises at the equator, by reason of the solar heat, and flows from thence toward the poles, has the diurnal motion due to the equatorial regions, and consequently greater than that of the parallels over which it passes. It must, therefore, from this cause, tend to return back again toward the equator, and will recede from it only till the two forces are in equilibrio, say in lat. 30° to 35°, or thereabouts. In like manner the surface air that flows from the poles, has a less diurnal motion than is due to the parallels over which it passes, and so ever tends from this cause to return again to the poles, and recedes from them only, say, to the parallel of 60°. A movement thus generated is probably further assisted by the condensation of vapor at the equator and the parallels of 60°, where the surface currents meet, producing an increase of temperature, and a diminution of atmospheric pressure.

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## 8. On the Occasional Luminousness of the Atmosphere at Night on the Andes. By Rev. George Jones, U. S. Navy.

In one of the volumes of Cosmos, reference is made to a luminousness of the atmosphere occurring once in Germany, when people were able to read small print at night. I have not been able to meet with any thing on that subject except this slight allusion to it by Baron Humboldt; but soon after my arrival at Quito, in Ecuador, in 1856, I noticed a similar phenomenon in the nightly atmosphere at that place. Both atmospheric and astronomical phenomena are brought out at that elevation of nearly 10,000 feet above the sea, with a very remarkable distinctness and individuality; as we might indeed expect when we know that Humboldt was there able to see his friend Bonpland, with his naked eye, at the distance of 17½ miles. Soon after I had begun to make observations on this luminousness at Quito, a foreign gentleman in that city, Col. Lanegan, who had taken part in the war for independence, said to me one evening, "Mr. Jones, what I hear about your observations on the zodiacal light explains to me something I have often noticed up in these regions,—I mean the very great brightness of the atmosphere at

night.* I remember being cheated by it once in a singular manner. I was travelling, and had stopped for the night at Machache, a day's journey, you know, south of this. My servant came to my room some time in the night — at what hour I do not know exactly, for I had no watch with me — and waked me with information that 'day was breaking.' 'Well,' I told him, 'saddle up, and we'll go on our journey.' He did so, and we started, and every thing around us was as distinct as at daybreak: but we journeyed on, hour after hour, and still day did not come. The atmosphere was quite bright, and every object on the road (in some places a very difficult one, leading among ravines and precipices, as well as every thing about us, was quite distinct. Then, after some hours of travelling in this manner, the air grew quite dark so that we could not see any thing; and after this darkness day at length broke. I judge, from the distance we travelled, that it was about one o'clock when we started from Machache."

This observation of Col. Langton led me to inquire of a Col. Stacey, an English gentleman then at Quito, who had also been an officer in their revolutionary struggles, and who had thus often had experiences of night marchings on the Andes. His reply to my question — whether he had ever noticed any thing of the kind — was: "O yes, often. Sometimes the night would be so bright that every thing was clear to the eye, and we had no difficulty in the roughest passes: and then again the night would be so dark that we could scarcely get along at all." Col. Stacey could give no particulars, but only remembered the facts in a general way.

I determined to make my own observations on this subject as precise as possible, and in such a manner as could leave no room for mistakes; and I selected such nights as were quite cloudy, or so much so as not to permit any suspicion of light from the stars. Some such nights would be so intensely dark that, on holding my hand upright twelve inches from my eyes, I could not see that it was there: indeed, no object would be visible. Perhaps the next night — perhaps, indeed, only a few hours after this intense darkness, all other things being equal, the sky equally clouded — the atmosphere would be so bright that I could easily distinguish objects even at a considerable distance. I was then

* Col. L. was wrong in his memories, but I as him proceed without interruption in his statement of facts.

enjoying the hospitalities of our Minister-resident at Ecuador, his Excellency Philo White, and was furnished by him with newspapers which I took out with me on these occasions, so as to try whether, without previously knowing what they were, I could make out any portion of their contents. The following is copied from my journal on the subject:

Quito, January 26, 1857, $9^h\ 30^m$ P. M. I have just been out on the azotea (open balcony), at the southern side of the house, in order to try how far I could read printed matter simply from the brightness of the atmosphere. The sky was clouded over, except now and then it cleared off in the zenith sufficiently to show a few stars, but they appeared dimly. There was no help from star-light, or, indeed, from any thing, except simply the light of the atmosphere. I first took out several newspapers, not knowing what they were, and read without difficulty the titles of the following: *Milwaukee Weekly News, Oneida Weekly Herald, The New York Herald, Weekly Journal of Commerce, Edinburgh Evening Courant.* In the case of the N. Y. Herald, and also most of the others, I was able to make out every letter distinctly. I afterwards took out several old Spanish books, of whose titles I had no distinct recollection, and tried them, with results similar to the foregoing.

February 14, $9^h\ 30^m$ P. M. This is a very *dark* night, though I can see no reason for the difference between it and the one noted above — this being cloudy as that was. When I hold my hand before me *vertically*, I cannot see it at all, at any distance from my face; if I hold it *horizontally*, I can make out the shape of the palm, and dimly of each finger. I took the above printed matter out with me, not expecting to be able to read it, but merely to make the experiment even with the former one. Could not make out a single letter. Could see something black where the Spanish book was — nothing more; and could do this only when it was held horizontally.

February 24. On looking out last night at 12 o'clock, I was struck with the remarkable luminousness of the atmosphere, and again made experiments in order to see how distinct printed matter would be to the eye. There were several newspapers lying about, of the titles of which I was not aware. These I took up and tried successively with the following results: *The Boston Statesman and Weekly Post;* the first part of the heading I could easily read, making out each letter readily

and quite distinctly; the lower line I could not read. *The Times for California*: this I could read distinctly, making out each letter. *The Weekly Herald*: could easily make out every letter. This heading was decidedly more distinct to the eye than on the 26th of January; the luminousness, consequently, was greater now than even on that occasion. *The National Intelligencer*: I could read this heading, and saw really what it was, but could not make out every distinct letter; the letters are much crowded together.

The sky, on this occasion, was much clouded over, except below the north towards the east, where a few stars finely showed themselves. Afterwards, at 2 o'clock, the sky had cleared considerably towards the east, the brightness of the atmosphere continuing. The luminousness was so great that it scarcely seemed to be night.

February 28. Last night was remarkably dark: sky cloudy: darkness almost utmost.

Ambato, sixty miles south of Quito. March 14. The atmosphere, for now so close nights previous to this, was very dark, — surprisingly so, though it was particularly bright. I made experiments at midnight with regard to reading. Went out at one of the most luminous of my doors, which looks towards the N. E., and, using care that none should be reading but the atmospheric brightness at that time, made experiments with newspapers as before. Results as follows. *The Journal des Débats*, and *New York Weekly Herald*: easily made out each letter at once. *Evening Post*: easily made out. *Washington Sun*, *Tribune*, *Times*: letters crowded, and could not read it so readily; could fully make them out, but some of them with the very closest. I could easily count the rows of tiles on the opposite house. The street lamps are too weak.

[illegible] the night, that is, from 12 to 2 o'clock, a portion of the sky was too much cloudy, and the stars there were out. Around the rest of the horizon the stars were almost entirely obscured. I could see that it will suit as atmospheric. I had been observing the meteoric light I shall have visible, I believed to be on account of the brightness of evening, etc.

March 15. In the early part of this evening the atmosphere was very dark, so that [illegible] was particularly bright, as at the same hour on [illegible] nights. This is a few hours [illegible]

Of this variously sized printing noticed above, as at the heads of the newspapers, that of the *Edinburgh Evening Courant* was the smallest; the letters there, small Roman letters, being a trifle more than a quarter of an inch in length; they are double lined, open and not full. If any person should think that it is nothing remarkable to be able to read such printing in the open air, at night, without extraneous aid, let him try such printing, or any other, and he will find how greatly he is mistaken.

. This subject of luminousness in the atmosphere seems to have forced itself very strongly upon the attention of the æronauts who lately made their ascent in the great balloon from St. Louis. Mr. La Mountain gives the following account:

"Throughout the night (July 1, 1859, — the moon set at $8^h$ $45^m$ P. M.) we were able, at all times, to distinguish the prairies from the wooded country below, even when at the altitude of two miles. We were floating in a sort of transparent vapor, which, without possessing any perceptible body, yet seemed to be made up of luminous particles. The effect of this light was very peculiar. It gave the balloon a phosphorescent appearance as though it were charged with fire. So powerful was this that every line of the netting, every fold of the silk, every cord and wrinkle was as plainly visible as if illuminated by torches, and I could, at any moment, tell time by consulting my watch. This phenomenon became more striking as we increased our altitude. . . . . That it is not a natural feature of night above the earth's surface, is shown by the fact that when Charles Green, Mark Mason, and Lord Holland made their famous nocturnal voyage from London to Weilburg, it was so dark that it seemed as if the balloon was passing through solid blocks of black marble."

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The materials in which they are found are the coarse drift, such as everywhere in the west occupies the summit of the country.

Between the State line and Lake Winnebago, the pebbles and boulders of the subjacent Niagara limestone constitute a large portion of the mass, with which sand and gravel are intimately mixed.

I have traced them 150 miles further in a northerly direction to the Wissakote or Brulè river. After passing beyond the sedimentary rocks above Lake Winnebago, the proportion of sand increases, and also the size and number of the boulders, which are mostly of igneous origin.

To get an idea of the appearance of the "potash kettle" country, imagine a region of drift moraines inverted. Instead of a surface thickly set with rounded hillocks, suppose it to be occupied by cavities of irregular size and depth.

If the grinder of a mastodon is reversed and impressed upon a piece of wax, the depressions which result, will represent the drift cavities as contrasted with drift elevations. In travelling through such a region the explorer frequently finds them so near together, that he no sooner rises

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Suppose the northern hemisphere during the ice period to have been covered with *nevè* to the depth of several hundred feet, as Greenland is now.

This field must have been first attacked by the heat of the sun on the side of the equator. Its southerly limit being at lat. 40°, it would be along this edge rather than upon the surface that it would be first melted. The conditions existing in all glacier regions would then be found, only the field would be a larger one. The alternate thawings of the day and freezings of the night would create a tardy, but a resistless, expansive force. On the north, the extent of the mass would be such

that in that direction there could be no movement, and the expansion must produce all of its effect in a southerly direction. Thus, so far as resistance in the rear gives rise to motion in front, the fixed mass of ice is equal to a central mountain chain.

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out of one than he is obliged immediately to descend into another, the diameter of which may not be more than twice or thrice its depth.

There is very seldom any water in the bottom, owing to the loose and porous character of the gravel drift. Boulders are seen at the bottom, on the sides, and on the surface around them.

When they are thickly set, as at the source of the Oconto river, and no hillocks between, the rim or edge between them is sometimes so narrow that large boulders have not base enough to rest upon, and tumble down the sides.

The internal slope is frequently straight like a funnel or inverted cone, but more often cup-shaped or curved in a manner correctly expressed by the form of a kettle.

In the prairie region of southern Wisconsin, timber grows within them, as it does on the adjacent lands, scattered in clumps or in separate trees, under the local name of oak orchards.

Further north, in the thickly timbered country between the Fox and the Wissakote rivers, the cavities are filled with trees.

Near the Wissakote, in T. 40 N. R. 18 E. (Wisconsin meridian), at an elevation of 800 feet above Lake Michigan, they are more broad and trough-like in form, the drift is more sandy, and small lakes, ponds, or marshes are occasionally seen at the bottom.

Of course the boulders and the gravel are here derived from the azoic and igneous rocks at the north. On the line of the survey for the "Chicago, St. Paul, and Fon du Lac Railroad," in T. 34 N. R. 17 E., on the north of the Peshattego river, at an elevation of 660 feet, the "kettles" are very numerous and sharply defined. Proceeding southerly, a series of them occur in T. 31 N. R. 17 E., about twenty miles north of the Oconto river, the summits of the country being 335 feet above Lake Michigan, and 913 feet above the ocean.

Those on the dividing ridge between the waters of the west branch of the Oconto and the Wolf rivers, in T. 32 N. R. 15 E., are at an elevation of 350 to 400 feet, and afford the finest instances of steep and well-defined cavities. While exploring that district in 1849, it occurred to me that these cavities could not be explained by the usual and well-known examples of aqueous deposits. Terraces and oblong ridges of sand or gravel might be formed by currents and eddies acting upon loose materials. It is not difficult to perceive how mounds, irregular elevations, and undulations could be thus built up by gradual accretion *above* the general surface.

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*perior, the sections of* Mr. Murray show alternations of greenstone, *quartzite, conglomerates* (both slaty and quartzose), and of crystalline limestone.

*Quartz veins cut the greenstone carrying sulphuret of copper.*

*The Huronian rocks are also regarded as metamorphic.*

On both systems the Lower Silurian strata are seen to rest unconformably and with little show of disturbance.

About 100 miles to the westward of Gros Cap are the Huron mountains of Michigan, rising through the Potsdam sandrock like the Gros Cap range in Canada, and to about the same elevation, 1,000 to 1,200 feet above the lake. Here is a central core of granite, with quartz veins and segregations not carrying metals in valuable quantities.

Along the shore of Lake Superior, from Presque Isle bay to the Huron islands, a distance of about fifty miles, the granite is protrusive, in many places disturbing the sandstone like the trap of Point Keweenaw. There are also entangled with the granitic masses beds of a fragmental rock evidently eruptive. On the south and west are the "iron rocks" folding around the westerly point of the granite, and making up into a field more than fifty miles in its shortest diameter. [illegible] formation at Marquette is a comparatively narrow belt [illegible] Lake Superior in the direction of Gros Cap. This tongue [illegible] shore is also bounded on the south by granite [illegible] Chocolate has its sources. Across the end of it [illegible] the Potsdam red and gray [illegible] with the non-fossiliferous rocks.

[illegible] Lake Superior to Green Bay. [illegible]

[illegible]

[illegible]

slates, trappose beds and roofing slates, mica slate, porphyries, sienitic greenstone, quartzite, serpentine, &c.

My field labels of 1849, 1850, and 1858, show the same rocks under the names of vitreous, and vesicular quartz, greenstone, talcose slate (green and yellowish green), chlorite slate, trap, feldspathic rock, hornblende rock, argillaceous slate, &c.

The granitic and sienitic portions are described by all explorers alike, except these rocks are intimately blended as is frequently the case.

Immense deposits of specular iron ore in various forms are found throughout the series, associated with marbles; the iron is sometimes magnetic, but in general is not so until it has been calcined. It is both fissile and compact like the other rocks; it takes also the fibrous form like wood, with a silky lustre, and the micaceous in shiny scales; in this respect the iron obeys the same laws of arrangement as the slates which take all these forms.

To the west of the Menominee, on the waters of the Peshattego, the Occonto, and the Wolf rivers, is an extensive field of coarse red and gray sienite, with enclosed patches of hornblende rock, sometimes slaty, but more often compact.

Along the boundary between the sienites and the azoic rocks, long narrow spurs run out from each into the other, alternating in such a manner that I have been unable to determine the order of superposition, if there is any, or to separate them as to age.

The Potsdam rests on both without disturbance; neither do I in this region find evidences of protrusion of the granites and sienites among the slates.

The great diversity of names bestowed upon specimens from the waters of the Menominee, led me to submit a full series of them to the test of analysis. I am not aware that the rocks of the Huronian and Laurentian have been thus examined as a system.

Those of my collection that are classed as marbles, and the specimens of iron ore, have not yet been completed. The marbles will probably prove to be dolomites and serpentines. Of the so-called magnesian slates and trap-like compact rocks, I made (15) FIFTEEN groups as nearly alike within themselves as the arrangement could be made by external characters only. They were analyzed for the Wisconsin Survey, by Professor J. L. Cassels, of the Cleveland Medical College.

Before presenting the results, I will state that the extensive green and dun-colored azoic slates on the waters of Bad river, south of La

Pointe, and which embrace immense beds of magnetic and specular iron ore, both slaty and compact, cannot be distinguished by the eye from those of the Menominee, the Matchegummi, and the Wissakote rivers.

There is also on the St. Louis river, west and north-west of Lake Superior, an extensive and heavy bed of green slates that underlie the trap beds of the north-west coast of the Lake. These slates graduate into hornblende rocks.

The trap rocks are here intercalated in great force with the Potsdam sandstone and conglomerate like the copper-bearing trap of Point Keewenan, and are therefore the same age.

Still further in the interior, about the sources of the St. Louis and Vermillion rivers, are granite, sienite, gneiss, and mica slate.

The area occupied by the azoic rocks of the American waters of Lake Superior is, therefore, very extensive. They are everywhere found beneath the oldest of our fossiliferous strata, and are also more ancient than the trap outbursts of the silurian epoch. They contain numerous but limited veins and nests of quartz, both vitreous and ferruginous, in which copper and iron pyrites are found in small quantities.

On the Bad river there are enclosed masses of sienite apparently contemporaneous with the schists as on the Oconto and Peshattego; there are in the sienite, enclosed bodies of slaty hornblende. The so-called magnesian slates pass everywhere into hornblende rocks, and in general they abut upon or are mingled with granite or sienite. In but few cases are they disturbed by upheavals, but appear in general to have remained quiescent from the date of their origin. The lamination has no apparent consonance with the bedding or stratification, which latter is very difficult to determine.

In the report for Wisconsin are tables in which the strike of these rocks is given in connection with the strike and dip of the laminæ where they are seen to be at all angles with each other. There seem to have been wrinkles and corrugations, but few uplifts.

The thickness of the system and its dip cannot, therefore, be given or even estimated with any reasonable approach to the truth.

To determine its physical geology with certainty, requires farther examination in districts very difficult of access, and to fix its mineralogical character requires a complete series of analyses.

Perhaps when this is done the same simplicity of composition may be found to hold among specimens broadly different to the eye, as is disclosed in the following table.

ABSTRACT OF THE ANALYSIS OF SPECIMENS OF THE AZOIC ROCKS. By Prof. J. L. CASSELS.

| No. of the group. | External Characters. | Locality. | Specific gravity. | Silica. | Alumina. | Protoxide of iron. | Lime. | Magnesia. | Manganese. | Moisture at 212 F. | Loss. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | Texture rough — color brownish black — compact, with a few crystaline faces — not affected by weather — general aspect igneous. | Menominee river, 6 miles above Kitson's bend. S. line of T. 39 N. R. 18 E. Wis., representing many localities. | 2.776 | 88.00 | 5.25 | 4.14 | 0.56 | 0.28 | 0.18 | 1.50 | 0.09 |
| 2 | Conchoidal— fine grained—flinty—dark yellowish-green small crystals of iron pyrites — aspect of novaculite. | | 2.608 | 92.00 | 3.40 | Sulphide 2.00 | 0.80 | trace | — | 1.45 | 0.35 |
| 3 | Tough conchoidal and splintery — dark green with whitish gray blotches — aspect igneous. | Above and below White rapids & other localities. | 2.730 | 84.00 | 6.80 | 4.38 | 1.20 | 2.28 | 0.30 | 1.00 | 0.04 |
| 4 | Sectile — fails to pieces in the weather — brick red with dun spots — magnesian aspect and feel. | Red Pipe Stone island and many other places. | 2.592 | 82.40 | 5.60 | Peroxide 6.35 | 1.40 | 1.80 | — | H. & C. 1.90 | 0.15 |
| 5 | Rough — sub-crystalline, not hard or brittle, tough — dark green and grayish white mottled. | Embraced, in No. 4. | 2.867 | 87.50 | 4.60 | 4.88 | 0.35 | 0.65 | 0.30 | 1.52 | 0.20 |
| 6 | Rough — few crystalline faces, tough and hard — dark green to black, under a magnifier mottled trappose aspect in the mass. | Pemene Falls, and two other localities. | 2.822 | 82.00 | 6.25 | 6.21 | 3.20 | 0.73 | 0.36 | 1.20 | 0.05 |
| 7 | Splintery and conchoidal—compact passing into slate — blackish green and brownish black — jointed. | Very numerous localities. | 2.836 | 76.00 | 3.60 | 12.00 | 4.80 | 2.00 | — | 1.35 | 0.25 |
| 8 | Coarse earthy — cut with a knife, gray, mottled with dark green, weathers darker with a stain of iron — magnesian aspect. | Sturgeon Falls, and other places. | 3.104 | 73.70 | 5.00 | 16.00 | 2.80 | 0.25 | 0.40 | 1.75 | 0.10 |
| 9 | Sectile and friable — soft, shining powder, yellowish brown — labelled silvery talc. | Sandy Portage. | 2.252 | 52.55 | 12.15 | 16.92 | 12.80 | 4.24 | — | H. & C. 1.08 | 0.34 |
| 10 | Fibrous and lustrous — cut by the nail — dark gray — greasy feel — powder greenish white. | Lower Bekuenesee Falls. | 2.782 | 61.21 | 11.20 | 24.35 | 0.85 | 0.42 | — | H. & C. 1.60 | 0.37 |
| 11 | Crystalline — very tough — rough fracture — dark green mottled — powder greenish white. | Head of the gorge, Upper Bekuenesee Falls. | 2.870 | 67.20 | 12.00 | 7.64 | 10.00 | 1.48 | 0.36 | 1.20 | 0.14 |
| 12 | Earthy — easily pulverized — dark green — small crystals of iron — powder greenish gray — in beds of a trappose cast. | Section 22, T. 42 N. range 34 west, Michigan. | 2.503 | 55.20 | 10.00 | 20.41 | trace | trace | 0.50 | H. & C. 3.88 | 0.01 |
| 13 | Splintery — flinty — laminated and jointed — powder pea green — hornblendic aspect. | T. 36 N., range 16, east Wisconsin. | 2.912 | 82.80 | 5.60 | 6.00 | 1.79 | 1.60 | 0.48 | H. & C. 1.25 | 0.08 |
| 14 | Compact — brownish gray — powder brownish white — feldspathic aspect. | T. 35, R. 15, E. Wisconsin, representing a large tract. | 2.604 | 90.25 | 2.76 | 3.38 | 1.65 | 0.88 | — | H. & C. 1.00 | 0.08 |
| 15 | Compact — fine grained flinty — jointed — bluish gray — weathers light gray — translucent at the edges — green — powder greenish white. | South-east part of T. 35, range 15, east Wisconsin. | 2.675 | 86.80 | 8.80 | 5.04 | 1.68 | 1.88 | — | H. & C. 0.80 | 0.20 |

To be final these analyses should be extended to all the forms and all the supposed varieties of the azoic rocks of the north-west, but those here given unfold new views in regard to their classification. Among them there is no instance of the presence of potash or soda, a fact which places them without the feldspar family in any of its numerous varieties. In the Laurentian, so far as I have seen the analyses, these alkalies are always present. If, therefore, the azoic slates are metamorphic, their derivation must have been from beds of a different composition and of different age from those in Canada.

In the Laurentian, magnesia is a prominent ingredient, rising as high as thirty per cent., but here, although it exists in small quantities, from a trace upward, it in no case reaches five per cent. Acting upon the theory of metamorphism, it is necessary to suppose two sedimentary systems, both older than the Potsdam, which have everywhere lost all vestiges of their original condition.

As to the modifying power of heat, it could extend no further than to a change of arrangement in the particles of the bases and to a diminution or increase in the gaseous constituents. We have in numerous instances on the American shores of Lake Superior a clear view of the contact between trap rocks, regarded by all as eruptive, and the Potsdam sandstone, where no material change has taken place. For hundreds of miles there are unaltered or slightly altered beds of this sandstone between beds of trap. In this region, therefore, the only positive evidence we have is against the idea that the intrusive rocks work a change in the structure of the sedimentary ones by heat alone. Is the fact of metamorphism so universal as to authorize us to infer it without evidence?

The oldest sedimentary beds in that region, which are among the most ancient of the palæozoic series, have been greatly disturbed by igneous outbursts without losing their stratification. Among the azoic slates the disturbances are few, and yet it is impracticable to determine with any approach to certainty, whether any order of superposition prevails among them. At the same time no adequate cause is visible whereby a stratification if it once existed, could have been destroyed.

In the Alps, where fossiliferous beds have undergone metamorphism, there is proof of the change, the same strata are traced laterally from one condition to the other.

In general, the metamorphic rocks are gneissoid, but the azoic slates do not contain the alkalies without which mica cannot be formed.

They are shown to be hornblende rocks, possessing great uniformity in the character and number of their constituents, though varying in proportions. The only alkali is *lime*. They are highly silicious, the silex in most cases in excess; and therefore in a free state like quartz, there is not magnesia enough to give rise to serpentine, in a single instance. The green and dun color seems to be due to silicates of iron, and not to chlorite, or to silicates of magnesia. There is not alumina enough to bring them into the category of clay slates, and the usual alkalies are wanting. The black portions receive their color from a small portion of hornblende and from the black oxide of iron.

There are between the Michigan and the Canadian series resemblances in the presence of crystalline limestone, and of ores of iron. Crystalline limestone also exists in the Huronian of Sir Wm. Logan, but it is not found in the copper bearing range of Point Kewenaw.

The presence of dolomite and marble is a fact favorable to the idea of metamorphism, but is it so conclusive as to do away with other facts of an opposite bearing?

Heavy beds of greenstone of acknowledged igneous origin are included in the Huronian and the Laurentian; on the waters of Lake Huron — are they any more of an intrusive character than the slates with which they are associated?

There are in the region of the Menominee instances of metamorphism at the edge of the Potsdam sandstone, where its outcrop rests upon the azoic slates. Those instances will be noticed in the forthcoming report for Wisconsin. The sandstone retains its original bedding and aspect, and in some cases ripple marks, but has at present the compactness, toughness, and color of vitreous quartz.

It is easily distinguishable from the quartz and quartzite of the slates, which is often vesicular, ferruginous, and without lines of bedding. The metamorphic quartz is, however, very limited in extent. It has the form of outliers resting on the azoic rock beneath, in which there are no signs of disturbance. There is, so far as I have seen, no evidence of the operation of heat arising from contact or the nearness of eruptive rocks. Unless the whole azoic system is eruptive and igneous the parts associated with the metamorphic quartz would not be so considered. Without going into the discussion of a theory to explain the limited changes effected upon the Potsdam sandstone by contact with the slates, I merely state that the agent is probably not heat.

The presence of so much iron, it seems to me, is adverse to the

theory of metamorphism. In combination it varies from two to thirty per cent. of the specimens analyzed, none of which were selected as ores, and few of them exhibited signs of that metal. The ores proper range from fifty to seventy-five per cent., and constitute a large portion of the system. It is necessary to have seen the number and extent of the exposure of iron slates and masses, to comprehend the quantity. The marbles are comparatively limited, but iron is universal. We know of no sedimentary formations so thoroughly charged with it. Iron and silex are the prevailing ingredients; alumina and lime exist in important proportions, but rising no higher than (12) twelve per cent. These rocks are associated in the closest physical relations to those that are universally considered as of igneous origin — why should they be separated? The igneous rocks are made up of silex, feldspar, hornblende, and mica; these are composed substantially of silex, iron ore, and hornblende. In the first, the alkalies are lime, potash, and soda; in the last, lime only, but the protoxide of iron, magnesia, and manganese act the part of bases in hornblende. Sienite, which is here far more widely diffused than granite, is composed of quartz, feldspar, and hornblende. The analyses, so far as they have extended to the azoic rocks in contact with the sienites, are composed essentially of hornblende, quartz, alumina, and iron; the last two as silicates. To form serpentine marbles from such materials, only requires an exercise of the well-known process of segregation and separation of like substances that appears in all rocks. The iron ores necessarily result from the concentration of the excess of the oxides present. Is the difference sufficient to authorize a separate and widely different origin to the two series?

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3. Recent Researches in the Devonian and Carboniferous Flora of British America. By J. W. Dawson, of Montreal, C. E.

[*Abstract.*]

1. *Devonian Land Plants of Gaspé, Lower Canada.* — The plants described occur in the Gaspé sandstones of Sir W. E. Logan, which are associated with beds containing marine fossils of Devonian age, and underlie the lowest carboniferous rocks. The forms noticed were as follows:

*Psilophyton* (n. g.). Lycopodiaceous plants, branching dichotomously, and covered with interrupted ridges or closely appressed minute leaves; the stems springing from a rhizome having scattered circular areoles sending forth cylindrical rootlets. Stems consisting of an axis of scalariform vessels surrounded by cellular tissue, and externally by a cortical layer of elongated wood cells. Fructification? in lateral masses protected by leafy bracts.

The typical species *P. princeps* is extremely abundant in the Gaspé sandstones, its rhizomes, which are usually from half an inch to an inch in diameter, completely filling some beds. In addition to the stigmaria-like areoles, they are covered with minute punctures, probably marks of ramenta. The stems are very slender and glossy, and curve circinately in vernation. The fructification appears to consist of cuneate clusters of bracts. The growth of this plant has in one place produced a layer of coal about an inch in thickness.

*P. robustius* is a second species, of which only a few examples were observed; its stems are thicker and bifurcate very frequently, while the ridges upon them are finer than on *P. princeps*.

Fragments of these plants have no doubt been the originals of many so-called fucoids, of the genera *Chondrites* and *Haliserites;* and when flattened the branchlets might easily be mistaken for fern leaves of the genera *Schizopteris* and *Trichomanites*. The rhizomes, if taken separately, would belong to the genus *Karstenia*.

*Lepidodendron*. *L. gaspianum* is a species with slender stems, and acuminate elliptical areoles with central leaf scars. The areoles are two lines long and one line broad, in stems two inches in diameter. The leaves are thick at the base, short, slightly ascending and then curving downward. This species is closely allied to *L. chemungense*, Hall, and *L. nothum*, Unger; and perhaps all may prove to be varieties of one species when more perfect examples are procured.

*Prototaxites* (n. g.). Woody trunks with concentric rings of growth and medullary rays. Cells of pleurenchyma not in regular series, thick-walled and cylindrical, with a double series of spiral fibres and traces of disks.

*P. Logani* is a species of which one example, eighteen inches in diameter, has been found. It is the coniferous tree described and figured in the proceedings of the Meeting of the American Association for 1856.

Fragments found with the above, indicate the existence of species of *Knorria* and *Noeggerathea*, and of plants allied to *Calamites*. The plant-bearing beds contain fish spines (*Machæracanthus*) *Beyrichea*, ripple marks, shrinkage cracks, and impressions of rain marks.

2. *Vegetable Structures in the Coal of Nova Scotia.* The author had so far perfected the methods of preparing specimens of coal by the action of nitric acid, as to be able to make out with great distinctness the tissues remaining, whether in the state of mineral charcoal or of compact coal. In the mineral charcoal he had found discigerous and scalariform tissues of *sigillaria*, *stigmaria*, *calamites*, and *conifers*, scalariform vessels of *lepidodendron*, *ulodendron*, and ferns, large spiral ducts, perhaps of some monocotyledenous plant. The discigerous and scalariform tissues of *sigillaria* and *calamites* and the scalariform tissues of *stigmaria* largely predominate. In the coarse, earthy coal, there is much debris of leaves and herbaceous plants especially of *Noeggerathea*.* The shining, compact coal consists principally of the flattened bark of *Sigillariæ*, with such portions of woody matter as escaped the subærial decay necessary to the production of mineral charcoal.

The general conclusions deduced were:—That the mass of the coal consists of the remains of the gymnospermous families *Sigillariæ* and *Calamiteæ;* that it was accumulated by growth *in situ* on swampy flats; that in the process immense quantities of the more perishable vegetable matters disappeared by decay; and that the growth of coal was exceedingly slow.

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## 4. Contribution to the History of the Laurentian Limestones. By Sir W. Logan, of Montreal, Canada.

Sir William Logan exhibited to the section, a map on which was delineated in detail on the scale of an inch to a mile, the distribution of some of the bands of crystalline limestone interstratified with the gneiss of the Laurentian series of rocks on the north side of the Ottawa river, about forty miles above Montreal. This, he explained, was a continuation of similar work shown at the Montreal meeting of the Association. By his recent exploration, two additional bands of limestone had been

* Perhaps *Cordaites*.

ascertained to underlie the lowest of those previously examined, the whole of the strata associated with these lower three, including the limestones, being supposed to be about 15,000 feet thick. These three bands are separated from one another by gneiss, a large portion of which is porphyroid or coarse-grained, the feldspar being almost wholly orthoclase; whereas, as was stated at the Montreal meeting, calcareous bands above them are largely associated with labradorite. Intercalated with the coarse and massive orthoclase gneiss, were frequent beds, which may be characterized as mica slate, and approaching the calcareous bands are beds of hornblende rock, and quartz rock, these latter, and sometimes bands of nearly pure white orthoclase, when immediately near the limestone or interstratified with it, being very often thickly studded with pink garnets; one of the beds of white and nearly pure quartz rock, which was traced for a mile and a half, presented a thickness of 1,000 feet. No instance of clay slates was met with.

These strata are exceedingly corrugated, and the outcrop of the limestone presents a multitude of sharp turns resulting from small plications subordinate to more important synclinal and anticlinal forms, the axes of which appear to run nearly north and south. Some of these axes have now been traced up the Rouge, a tributary of the Ottawa, for a distance of fifty miles in a straight line.

Although the Laurentian series has hitherto been considered azoic, a search for fossils in them has not been neglected. Such search is naturally connected with great difficulties. Any organic remains which may have been entombed in these limestones, would, if they retained their calcareous character, be almost certainly obliterated by crystallization, and it would only be through their replacement by a different mineral substance that there would be a chance of some of the forms being preserved. No such instances had been observed on the investigations of the Rouge and its vicinity, but from another locality in the Laurentian formation, Mr. John McMullan, one of the explorers of the Geological Survey, had obtained specimens well worthy of attention. They consisted of parallel or apparently concentric layers resembling those of the coral *Stromatocerium*, except that they anastomoze at various parts; the layers consist of crystalline pyroxene, while the interstices are filled with crystallized carbonate of lime. These specimens had recalled to recollection others which had been obtained from Dr. Wilson of Perth some years ago, and had not then been regarded with

sufficient attention. In these similar forms are composed of green concretionary serpentine, while the interstices are filled with white crystallized dolomite. If it be supposed that both are the result of mere unaided mineral arrangement, it would seem strange that identical forms should result from such different minerals in places very far apart. If the specimens had been obtained from the altered rocks of the Lower Silurian series, there would have been little hesitation in pronouncing them to be fossils. The resemblance of these forms to *Stromatocerium* from the Birdseye limestone, when the coral has been replaced by concretionary silica, is very striking. In the pyroxenic specimens, the pyroxene and the carbonate of lime being both white, the forms although weathered into strong relief on the surface, are not perceptible in fresh fractures until the fragments are subjected to an acid, the application of which shows the structure running throughout the mass. Several specimens of these supposed fossils were exhibited to the Section.

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5.* Remarks on the Discovery of a Terrestrial Flora in the Mountain Limestone of Illinois. By A. H. Worthen, of Springfield, Illinois.

In the autumn of 1851, while engaged in constructing a section of the Ohio bluffs on the Illinois shore, from the mouth of the Wabash to Cairo, a bed of shaly sandstone was found in Pope county, intercalated in the upper Archimides or Chester limestone, containing terrestrial plants in a fine state of preservation; and subsequent investigations have brought to light other localities where similar fossils may be found occupying the same geological horizon.

These fossils afford us the first evidences yet obtained, of the existence of terrestrial plant-bearing conditions in the valley of the Mississippi, at a period long antecedent to the true coal-bearing epoch.

The Devonian beds which have afforded remains of terrestrial plants in New York, Maine, and Nova Scotia, yield at the west only marine organisms; and it is not till we reach the mountain limestone series in

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* Abstract from the First Annual Report of the Illinois Geological Survey.

the ascending order, that we find a well-marked flora of a terrestrial type.

The Chester limestone usually consists of three subdivisions,—an upper and lower limestone, separated by a bed of arenaceous or calcareo-argillaceous material, and it is from the latter bed that the fossils now under consideration have been obtained.

The upper limestone, which immediately overlies and rests upon the plant-bearing bed, is a massive gray limestone in regular beds, with argillaceous partings between the layers, and attains an aggregate thickness of forty to sixty feet. It is filled with well-marked mountain limestone fossils, among which are the screw-shaped Fenestella or Archimides, two or three species of Pentremites, and among them the P. obesus of Lyon, the most gigantic species at present known; Spirifer incrassatus, Athyris sub-quadrata, and A. sub-lamellosa of Hall, together with several undescribed species of Mollusca and Crinoidea.

The fossils in this limestone are all specifically distinct from those of the coal-measures, not a single species having been yet identified as common to both. We are thus enabled to establish, in a most satisfactory manner, the geological horizon to which these plants belong.

We find among them representatives of the genera Stigmaria, Sigillaria, Knorrhia, and Lepidodendra, but of species quite distinct from those at present known in the coal-measures of the adjacent region. They do not appear to have been drifted from any considerable distance, but indicate the proximity at this period of adjacent land,—the coast of some ancient continent or island shore, the position and boundaries of which cannot at the present time be determined. The fact, however, that the whole sub-carboniferous series thins out rapidly to the north and east, renders it quite probable that this ancient coast line may have been in that direction.

The paleontological bearing of this discovery we shall not attempt to discuss at the present time, as the collection will soon be placed in the hands of a competent fossil botanist for investigation, and we confidently expect the localities already known will afford the material for an important contribution to the fossil flora of the palæozoic era.

## 6. Observations on the Geology of the Rocky Mountain Chain in the vicinity of Santa Fé, New Mexico. By William P. Blake, of New Haven.

[ABSTRACT.]

Very little was known of the rock formations of the region about Santa Fé until the visit of Dr. Wislizenus and of Lieutenants Abert and Peck, in 1846–47. Their collections and observations served to show the presence of Cretaceous formations in the valley of the Rio Grande, but were not sufficient to determine the ages of other formations. In 1853–54, Mr. Jules Marcou crossed the mountains and made known the existence of formations of the Carboniferous period on the eastern slopes of the Santa Fé range. Having made a tour of exploration through the region in 1857, I present, in brief, some of my observations.

The Santa Fé mountains are a par tof the great Rocky Mountain system. From a few isolated points or knobs which scarcely appear above the general level of the broad plateau, a few miles south of Santa Fé, they gradually rise to a majestic altitude — from 10,000 to 13,000 feet — and extend northwards in a succession of lofty ranges and peaks to the head-waters of the Arkansas. The city of Santa Fé is situated at the eastern base of the range, at an altitude of nearly 7,000 feet above the sea. At this point the central axis of the mountains is formed chiefly of metamorphic rocks, probably Silurian and Cambrian and older, consisting of gneissose and micaceous slates, with large numbers of intersecting granitic veins, in which a pink or red feldspar is very abundant. A ridge near Taos is not so highly metamorphosed, and the rocks are very slaty. A hornblendic slate, containing garnets, very similar to that found at Hanover, New Hampshire, occurs here. On the western slope of the chain the metamorphic slates are overlaid by strata of the Carboniferous period, and possibly by Devonian beds. The low hills and ridges directly east of the city, and at the base of the high ridges, are composed of Carboniferous strata, overlaid by a deep red soil and deposits of detritus or drift from the mountains. It is possible that strata of Permian and Triassic age also occur. In a ravine one mile east of the plaza the strata are exposed, and

consist of alternating beds of gray sandstone with beds of bluish gray and reddish limestone, of varying thicknesses, and a coarse, ferruginous red sandstone at the base. The limestones are fossiliferous, containing producti, spirifers, althyris, and encrinites, characteristic of the coal-measures. The stratification is very regular, and inclines to the west. The lower beds of sandstone show the action of varying and rapid currents at the time of their deposition, and the surfaces present fine ripple marks of large size. A short distance south of this exposure there is a quarry in the limestone, from which blocks for the construction of the new hall for the territorial legislature were obtained. The limestone is very hard and full of upper Carboniferous fossils.

A quarter of a mile east, the Carboniferous strata are seen to rest upon the upraised edges of the metamorphic slates, at an angle of forty degrees. Here we find an alternation of strata of sandstone, shales, and limestone, with bituminous layers, and probably in some places seams of coal. The limestone beds are highly charged with encrinites and corals. At another point, near by, in an exposure of less extent, a similar succession of strata were found, and with them a bed of impure bituminous coal, from one to two feet thick. Beyond, in the same ravine, is a great body of bluish and black shales, with a coal seam at the base. Several of the outcrops have been dug into, to obtain coal, but it had not been found in a seam thick enough to be worked with profit. The shales and coal were so much broken down at the exterior, that it was not possible to obtain a correct measurement of the thickness of either. The coal is seamed and divided by thin plates of selenite, and the shales are stained by oxide of iron, and a strong sulphureous odor indicates the decomposition of pyrites. A large number of fossils were obtained here, and beautiful impressions of ferns and coal plants were found in the shales. Specimens of these have been examined for me by Prof. Leo Lesquereux, who identifies two species, *Cordaites corassifolia* and *Sphenopteris latifolia*, both of which indicate the synchronism of the Santa Fé coal-beds with the anthracite and Illinois coal, placing its horizon with the Freeport or Pomeroy coal of Ohio, or the first below the Mahoning sandstone. It thus becomes probable that in the sandstone beds of the sections observed we may have the equivalent of the Mahoning sandstone.

*Anthracite Coal.* — About twenty-seven miles south-west of Santa Fé, on the flanks of the Placer or Gold Mountains, there are workable

beds of coal, and specimens which I procured are excellent anthracite. Several tons of it were taken out a few years since and carried to Santa Fé, but its characters and the methods of igniting and burning it not being understood, it was not liked. The quality is excellent, and it cannot but be very valuable in that region where timber is scarce. It is of especial importance to the country for its bearing upon the question as to the location of a railroad route to the Pacific. Here is, in all probability, an inexhaustible supply of the most appropriate fuel for locomotives. The strata with which this bed occurs are probably the prolongation of those at Santa Fé, but there was not an opportunity to examine them closely.

On the Galisteo, about fifteen miles south-west of Santa Fé, there are extensive outcrops of ferruginous and yellow sandstones, associated with beds of black shales. Beautiful exhibitions of diagonal stratification and ripple marks are abundant.

*Eastern slope of the Mountains.* — The formations of the eastern slope were examined at several points along the road from Santa Fé to Fort Union, and beyond to the Puerto. After passing the granitic axis the first stratified rock is a dark chocolate-colored sandstone, dipping east, and a sandy conglomerate. A bed of limestone crops out beyond, but could not be examined. In the Great Cañon stratified rocks on each side are well exposed to view, and are evidently in bold flexures, which die out as the distance from the mountains increases. No fossils were obtained here, but the formations at the base are believed to be Carboniferous, while those above are possibly Permian and Triassic.

Beyond, to the east, the strata are nearly horizontal, and form the table lands about the Pecos Valley. The escarpments of these table lands along the broad valleys of erosion, present beautiful sections of the rocks as well as the most picturesque views. The bluffs, which rise to the height of from four to six hundred feet, exhibit a succession of white, gray, and red sandstones, with red shales and marls, and here and there a layer of snow-white gypsum.

Upper Carboniferous limestone is exposed at Bernal Springs, where I obtained characteristic fossils, identical in appearance with those obtained by Mr. Marcou at the Pecos villages in 1853. This is overlaid by a great thickness of red and gray sandstones, apparently without fossils, which may be regarded as either Upper Carboniferous, Permian, Triassic, and Jurassic, or all of these, for the strata are conformable, and

there is no line of demarcation. The color is an unimportant characteristic, for the rock is frequently red without and white within, and parts of a bed are often white and others red, the coloration being due in part to the infiltration of ferruginous water.

*Rocks at the Puerto.* — Beyond Tecalote there is a second axis of granitic rock similar to that at the Santa Fé range. Stratified rocks are uplifted on its eastern flank, and form a belt of low hills through which there is a remarkable pass or cut called the Puerto, or gate, leading out upon the broad prairies. There is here a fine section of sandstones and shales, the latter being olive-green, and containing beds of nodular limestone. These strata by alteration would form red shales and bands of gypsum. They are probably the equivalents of the formations about Pecos.

*Absence of Lower Carboniferous or Mountain Limestone.* — In all these sections I failed to recognize any well-defined bed of limestone at the base of the coal-measures containing fossils characteristic of the sub-carboniferous limestone. It appears to be absent at the localities visited, the coal-measures resting directly upon the upturned edges of the older rocks, though it is possible that the thickly bedded red sandstone will be found to be the equivalent of the Devonian.

It is certainly very interesting to find beds of coal so far west, characterized by fossils identical with those of the Appalachian coal-field, and at an elevation of from seven thousand to twelve thousand feet above the sea, for it is probable that in places the coal extends to the very summit of the mountains with the associate rocks, which are known so to occur.

The beds of limestone, so far as seen, are thin, not exceeding forty feet in any one bed. It is not possible now to assign the thickness of the whole series of the rocks which must there be referred to the coal-measures, but it is probably less than 1,000 feet.

*Cretaceous.* — No fossils or other indications of Jurassic rocks were observed. Cretaceous strata were not seen along the Santa Fé road until the Puerto was passed beyond Fort Union. This formation presents bold bluffs along the north fork of the Canadian, with beds of slaty limestone containing inoceramus.

*Volcanic Rocks.* — The table lands of the Rio Grande, especially those on the west side, at the base of the Sierra Madre, are generally capped by horizontal layers of basaltic lava, forming mural faces along

the streams and cañons. These are well described by Messrs. Peck and Abert, by Wislizenus, and by Marcou. On the eastern slope of the mountains, broad lava plains are seen in the neighborhood of Fort Union, and lava is found far out upon the prairies in the isolated mounds known as Rabbit Ear and Wagon Mound. The former has the appearance of a volcanic cone, now extinct, but with the crater well defined.

*Mineral Resources. — Gold Mines.* — The mineral resources of the region are extensive and varied, coal, iron, copper, lead, gold, and silver being found in quantity; but having already made a communication on this subject to the Boston Society of Natural History, no further reference is necessary. I would, however, again call attention to the probability that the gold placers of New Mexico, so long known and worked with success, are probably connected with the recently discovered mines at Pike's Peak and on the head waters of the Arkansas.

The examination of the Carboniferous strata of the Rocky Mountains led me to conclude that the wide expanse of the great Carboniferous sea which covered the continent, was broken at intervals by islands along the course of the Rocky Mountain chain, and that we there had shores from which in part the materials for the coarse-grained thickly bedded sandstones were derived; and that, as in the Appalachians, the sandstones of the coal-measures predominate in bulk over the limestones, which last may be believed to gradually increase in thickness towards the Mississippi and southerly, while the sandstone beds thin out. Thus, while we find the coal-measures thinning out from a thickness of six to ten thousand feet in Pennsylvania, to about seven hundred or a thousand feet in Missouri, we will probably find a point of maximum thinness somewhere in Eastern Kansas, and that there is a gradual increase of thickness, of the sandstones at least, westward toward the line of the Rocky Mountains. The present great elevation of the strata, 12,000 feet above the sea at some points, may be referred not only to a great continental elevation, but to local uplifts and plications. This last is an interesting point — the existence in the distant and central chain of the Rocky Mountains of a geological structure, corresponding to the plications and flexures of the Appalachians.

In regard to the existence of Permian strata and those of the secondary period, I wish to be fully understood. The only horizons I have identified by fossils are the Coal-Measures and Cretaceous. Between

these, in that region, there is a series of strata in which we have, in all probability, representatives of the Permian, Triassic, and Jurassic formations — a probability rendered strong by the recent discoveries of Permian strata by Dr. Shumard in the Guadalupe mountains, much further south in the Rocky Mountain system, and in Kansas by Messrs. Meek and Hayden, and by Swallow and Hawn; the former have also brought Jurassic fossils from the Black Hills in the north.

There is no reason to change the opinion advanced at the Albany meeting in August, 1856, that the strata along the Canadian, referred to the Jurassic period by Mr. Marcou, are in reality Cretaceous; while it is certain that the formations of the broad expanse of the Great Plains between the head waters of the Canadian and the settlements of Eastern Kansas are *Cretaceous*, and more modern, rather than Jurassic.

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## 7. Relation of the River Terraces of Southern Ohio to the Drift and Drift Theories. By E. B. Andrews, of Marietta, Ohio.

The valleys of southern Ohio are all valleys of erosion. These valleys extend in all directions among the hills, deflecting in very waywardness, to every part of the compass. With the exception of those of the largest rivers, they have all been formed chiefly by the drainage of those rains which have fallen upon the immediate surface. The innumerable lateral valleys extending in every possible direction could only have been eroded from the horizontally stratified rocks by agencies local in their origin. These agencies have, with unconscious art, rounded all the hills and made them beautiful.

In the valleys of the Ohio and its larger tributaries, from the northern side, are found, at an elevation of about eighty feet above low-water mark, a large number of terraces, composed of stratified sands and gravel, with an occasional seam of fine clay. These terraces group themselves into a series, and invite the attention of the geologist. From instrumental examinations made, these terraces are found to correspond in elevation, allowance being made for the natural fall of the stream.

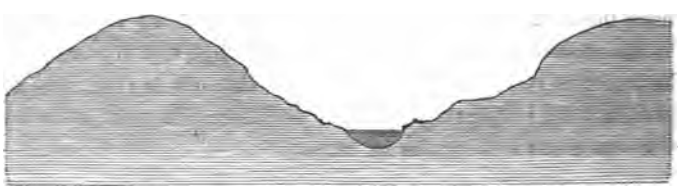
They were all formed during an epoch which may be called the

terrace epoch, and in which the waters of the streams were much higher than at present.

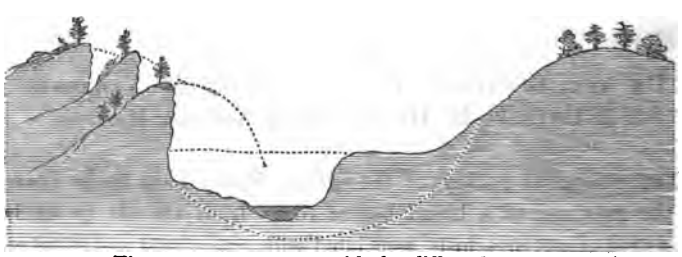
As the materials of the terraces came chiefly from the drift, once deposited in the central and upper part of the State, the excess of water, beyond the present natural drainage, must have been derived from the north. In addition to drift materials from north of the lakes, we find in the gravel, Devonian fossils from the northern part of Ohio, and in the terraces of the more eastern streams, pebbles of coal from our Carboniferous rocks.

So far as I am able to ascertain, there are no terraces whatever along any of the streams arising south of the Ohio, nor on any streams on the northern side whose head waters are not within the limits of the drift.

That the high waters of the terraced streams were not always turbulent and furious, is seen in the fact, that sometimes they remained in a quiet state long enough for the fine clay seams, now found in the terraces, to be deposited. But that they sometimes flowed in strong and fierce currents is evident from the very remarkable effects produced upon the adjoining hill-sides. All the hills were originally rounded. Along these terraced rivers we often find the half of these rounded hills gone. On the river side is a cliff, but the opposite side always slopes down into some valley, almost as deep as that of the river itself.



The hills as left by the original eroding forces.



The present appearance with the cliffs and terrace.

It seems probable that when the waters flowed at the high terrace level, they swept strongly against the hills, and, undermining them, caused the many beetling crags we find.

The question at once arises, Whence came these floods of water, now flowing in furious currents and forming the cliff, and now quietly resting and depositing upon the bottom the fine sediment which constitutes the clays?

They came from the north, but subsequently to the drift deposit. If we accept the glacial theory of the drift, then these waters may have flowed from the melting of vast bodies of ice.

If we adopt the theory of an ocean and icebergs floating in it, depositing the drift, then we must suppose that subsequently to the drift there was a drainage of this ocean through the valleys of southern Ohio. The absence of all recent marine organisms in the terraces would seem to militate against this supposition. But a greater difficulty is to believe that an ocean could maintain itself half-way down the great slope extending from northern to southern Ohio. This slope existed then exactly as it does now, as is proved by the innumerable valleys of erosion draining this area in a southern direction. Now could any other drift agency than ice operate half-way down a slope without extending further? In New England, where the land has been generally depressed below the ocean level, it may be difficult to determine what is due to purely drift agencies and what to oceanic agency; but in Ohio we have the drift entirely undisturbed except by a few rivers carrying down the smaller pebbles, and the sands and clays, and depositing them in the stratified terraces. The drift thus exhibited in Ohio, and separated from other perplexing questions, would seem to point to ice rather than to water and icebergs, as its cause.

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## 8. On the so-called Talcose Schist of Vermont. By Charles H. Hitchcock, of Amherst, Mass.

The geological surveys of the various States have made known to us the existence of a broad belt of rocks from Canada to Alabama, consisting of green schists associated with gneiss and gneissoid rocks.

The green schists have been denominated *talcose*, both in New England and further south. In our remarks, however, we would not be understood to affirm a non-magnesian character to any of these rocks south of New England, because we have neither seen or examined them. At the same time, there is a strong probability that their mineralogical characters are the same; unless, perhaps, the disagreement existing as to their age be full proof of their difference in composition, etc. For the Canada survey pronounce them metamorphic rocks of Silurian age, while the Pennsylvania survey regard them as Pre-Silurian, or Hypozoic.

First of all observers in this country, to our knowledge, Mr. T. S. Hunt, of Montreal, has stated that certain rocks of this belt at its northern prolongation, which would be regarded by most observers as a highly talcose schist, are almost destitute of magnesia, alumina being present in great force in its stead. An analysis of one of these schists from St. Marie, gave

| | |
|---|---|
| Silica, . . . . . | 66.70 |
| Alumina, . . . . . | 16.20 |
| Per. ox. iron, . . . . | 6.90 |
| Lime, . . . . . | .67 |
| Magnesia, . . . . | 2.75 |
| Alkalies (by difference), . . . | 3.68 |
| Water, . . . . . | 3.10 |
| | 100.00 |

Upon the Chaudiére river near Quebec, Mr. Hunt found specimens of pholerite in the crevices of the ledges, whose composition was about forty-six per cent. silica, thirty-eight per cent. alumina, and fourteen per cent. of water. To pholerite and pyrophyllite, both hydrated silicates of alumina and quartz, Mr. Hunt has referred the composition of the so-called talcose schists.

The same gentleman found that the clay-slates, or roofing slates, in the vicinity of these talcose schists had essentially the same composition as the talcose schists, excepting that they contained a larger percentage of alkalies, but never more than seven or eight per cent.; the talcose schist that he analyzed giving nearly three per cent. by difference. Hence he supposes that the talcose schists are formed from the clay slates by the process of metamorphism, whatever that may be.

He supposes that the mineral pholerite has been in a state of solution, "produced during the decomposition of the clay slates, which are made up to a large extent of the veins of feldspathic rocks. These slates are slowly giving up their alkalies to infiltrating waters, and are thus being converted into kaolin." He adds, "a great portion of the talcose slates of the Alleghany range, especially those associated with the gold deposits, throughout the eastern part of North America, are derived from the alteration of clay slates, and must be aluminous in their composition. It will be well for the future to distinguish them, on account of their lustre, by the name of *nacreous slates*, or *nacreous schists*."

These observations induced us to examine these same rocks, as they occur in Vermont in a more highly metamorphic state, to ascertain whether their character is magnesian or aluminous. We selected specimens from four different localities; such specimens as were apparently the best characterized talcose schists in the State. The results in every case agree with those of Mr. Hunt. The analyses were kindly performed by Mr. G. F. Barker, of Boston, a careful chemist, who has given the following account of them:

"*Talcose schist* (so-called), Roxbury. This rock is schistose, friable, of a greenish gray color; fissile; sp. gr. 2.72. The analysis gives the following result:

| | | |
|---|---|---|
| Silica, | 69.90 | |
| Alumina and iron, | 20.00 | |
| Lime, | 1.51 | |
| Magnesia, | 1.80 | |
| Soda, | 2.33 | 3.78 |
| Potassa, | 1.45 | |
| Lost by ignition, | 2.40 | |
| | 99.39 | |

"From the analysis it will appear evident, that the name 'Talcose' applied to this rock is a misnomer; there can be neither talc nor chlorite in them, for both these contain from thirty-two to thirty-six per cent. of magnesia. Mr. Hunt has proposed the name of nacreous slates, from their lustre; the small proportion of water in this rock would seem to indicate some other mineral than pyrophillite, and the total absence of any exfoliation under the blow-pipe, would tend to confirm this impression.

"We would say of the position of the specimen analyzed, that it is from the centre of the talcose schist formation, and was taken from the vicinity of the bed of verde antique, which is decidedly magnesian in its character. We selected this specimen as the most likely of any in the whole State to contain magnesia.

"*Talcose schist*, from Pownal. This is another schist of the same general character as the preceding. Its color is bluish gray; not as friable, but its feel is more unctuous. In dissolving the fusion with carbonate of soda in hydrochloric acid, hydrosulphuric gas was evolved, which blackened a solution of acetate of lead; yet the mineral, when treated with strong nitric acid, and boiled, gave no reaction for sulphuric acid, as would be the case had there been sulphurets present. The analysis was as follows:

| | | Oxygen. | |
|---|---|---|---|
| Silica, | 42.90 | 22.73 | |
| Alumina and iron, | 42.20 | 19.71 | |
| Lime, | .78 | .22 | |
| Magnesia, | 1.98 | .79 | |
| Potassa, | 5.24 | .89 | 7.22 |
| Soda, | 1.33 | .34 | |
| Loss by ignition, | 5.60 | 4.98 | |
| | 100.03 | Alkalies, 6.57 per cent. | |

"That this is a distinct rock from the last, is shown by the differences in the silica and alumina, there being twenty-seven per cent. less of silica, and twenty-two per cent. more of alumina. The specific gravity is greater, being 2.90; the magnesia remains nearly the same, while the alkalies and water have doubled. An analysis of talcite (Dana's Mineralogy, 4th ed. p. 509) by Tennant, strikingly agrees with this in composition. The specimen used was from Wicklow, Ireland. The oxygen ratios in this specimen will be seen to be nearly 1 : 3 : 3 (that is, 1 : 2.7 : 3.14) giving the formula $(\ddot{R}.\ \ddot{R}.)\ \ddot{S}i$."

The specimen analyzed is an ordinary sample of a somewhat decomposing schist, which is called *magnesian slate* by Prof. Emmons; belonging to what he has called the Taconic System. Pownal is the first town north of Williams College; and as it is so near the centre of his system, we think he must admit it to be from his magnesian slate. We have the impression that Dr. Emmons had an analysis made of this rock, wherein there was a large per cent. of magnesia; but we were unable to find it at the time of writing.

"*Talcose schist*, Middlesex. This is another of the so-called talcose schists, perhaps more properly *talcoid*. It is less schistose and more slaty than the preceding specimens; color, olive green; oder, argillaceous; composition,

| | | |
|---|---|---|
| Silica, | 64.10 | |
| Alumina and iron, | 23.50 | |
| Lime, | .84 | |
| Magnesia, | 1.98 | |
| Potassa, | 3.70 | 5.90 |
| Soda, | 2.20 | |
| Loss by ignition, | 3.60 | |
| | 99.92 | |

"This resembles more closely the analysis of the Roxbury specimen, the silica being a little less and the alumina a little more than in that rock. The proportion of the composing mineral varies, being mixed with extraneous matter. Scattered through it are octahedral crystals of magnetic oxide of iron. These were carefully removed by the magnet from the portion of the rock submitted to analysis. The presence of these rendered the determination of the specific gravity inaccurate. It is less friable than the preceding; has more tenacity; before the blow-pipe it is unaltered; a thin sliver, however, being softened on the edges."

The geological position of this specimen places it at the eastern boundary of the formation, and probably it is not far from the base, as the dip is west, the rock resting upon the roofing slate of Montpelier. We might say that it is intermediate in its character, between argillaceous slate and talcose schist.

"*Novaculite*, Irasburg. This is a mineral, very homogeneous in composition, fracture rough, structure amorphous, translucent, of a dirty gray color, streak white, specific gravity 2.65. A careful analysis gave the following composition:

| | | | | |
|---|---|---|---|---|
| Silica, | 78.70 | | | |
| Alumina, | 12.80 | | | |
| Iron, | trace | | | |
| Lime, | 1.23 | | .35 | 1.94 |
| Magnesia, | traces | | | |
| Potassa, | .89 | | .15 | |
| Soda, | 5.57 | | 1.44 | |
| Loss on ignition, | .60 | | Alk. 6.46. | |
| | 99.79 | | | |

"This would make the oxygen ratio, 1 : 3 : 21, or (RO, $R_2$ $O_3$), 7 $SiO_3$.

"Though called on the label talcose slate, its physical and chemical characters are widely different. Of magnesia there is but a trace, and the loss by ignition is small. Before the blow-pipe it becomes white and opaque, is infusible, and colored the flame yellow; with cobalt solution it gives a fine blue."

This is the most unctuous of all the specimens analyzed, and according to this supposed criterion for the presence of magnesia, a large per cent. is indicated. A rock of this character is very common in the talcose schist formation, and in the catalogue of the Vermont State Cabinet it receives the name of novaculite schist. It is also abundant among the Hudson river shales.

Observing that upon the Canada geological map the same color was given to the Oneida conglomerate about Burlington, Vt., and to the talcose schist formation, we were led to make an examination of the sandstone of the former deposit. A specimen which had a great excess of silica was used (of which variety there is but a small proportion in the whole formation), with the following result; analyzed by Mr. Barker.

"Specific gravity, 2.65; color, dark red, — on ignition it changed color, becoming blacker; a hard, compact sandrock. Composition:

| | | |
|---|---|---|
| Silica, | 83.30 | |
| Alumina and peroxide of iron, | 8.70 | |
| Lime, | 1.12 | |
| Magnesia, | .10 | |
| Potassa, | 4.59 | 5.04 |
| Soda, | .45 | |
| Loss by ignition, | .80 | |
| | 99.06 | |

"The excess of silica is due to sand present. It could not be separated, as the mineral is not decomposable by acids. The oxygen ratio of the basis is 1 : 3."

With the exception of the excess of silica, which is only occasional, both in the Oneida conglomerate and the talcose schist formation, the proportions of the other components agree. Hence it is not unlikely that this Oneida sandstone may, by metamorphosis, change into this so-

called talcose schist; so that we may have two sources for the schist; from clay slate and sandstone.

There is another consideration connected with this subject, of great importance. Associated with the aluminous schists is gneiss. They are not only interstratified, but the one seems to pass into the other by degrees. For instance, the belt of aluminous schists in the northern part of Vermont is several times as wide as the same formation in southern Vermont. But by including two belts of gneiss with the talcose schists in the southern part, the whole width corresponds to that in the northern part. In southern Massachusetts, the same belt of talcose schist entirely disappears, to be replaced by gneiss; but again in Connecticut, we believe, the talcose schist reappears. Thus a rock disappears and reappears, while the strata have the same uniform dip and strike, and it is difficult to discover lines of demarkation between them. By recurring to the analyses, we find that the composition of all of them corresponds with that of gneiss, also. For the principal constituents are silica, alumina, and alkalies, and the latter are as largely developed in the schists as in several analyses of gneiss that we have consulted. Hence, then, we have another stage of metamorphism. First there is a simple sediment; if coarse, it is sandstone; if fine, clay slate. A change takes place, called metamorphism, — the effect of heat and water combined, — which converts these sediments into nacreous or aluminous schists. The continued and probably intensified action of the same causes, converts these schists into gneiss, or feldspathic rocks. And were we to imagine a still greater increase of temperature, this gneiss might melt into granite, and the whole process of the strongest metamorphosis be seen. If this is so, it is possible that we have instances of the last stage of the metamorphosis in occasional hills of granite in the talcose schist formation — as in the towns of Plymouth and Braintree, Vt.; and in the gneiss of the Green Mountains, in the towns of Stamford, Sunderland, Stratton, etc.

The talcose schist formation, which we have been describing, lies upon both sides of the principal range of the Green Mountains, especially upon the east side. But the Green mountain range itself, in Massachusetts forming the Hoosac range, and in Vermont extending through the State, is composed of gneiss and gneissoid rocks. In northern Vermont, in the range of Mansfield Mountain and Jay Peak, it becomes more talcose, but with shining crystals of feldspar appearing in the

edges of the strata. We never should have regarded this rock in northern Vermont other than talcose, had we not examined the formation along its axis, and noticed the gradual change into talcose schist, with crystals of feldspar. In several localities in this vicinity this rock may be called a talcose sandstone, whose sedimentary character has nearly disappeared, in consequence of the metamorphic action. Thus, in the principal range of the Green Mountains, may we see another example of the change of talcose schist, and perhaps sandstone, into gneiss.

We say *talcose schist*, for two reasons; first, the universality of the non-magnesian character of this rock, though probable, ought not to be considered as settled by five analyses. Almost every stratum of this rock presents some variation from the adjacent ones. It may be that a large number of these will yet be found to be magnesian, while many are aluminous. If so it should appear at some future day, it would be much better for the cause of science that we retain the old name, talcose, for the present, and at the same time express the conviction, that probably there is no magnesia present in any of it. Such a scientific conservatism is always safe. And secondly, the analyses of some of these specimens show that they agree with the mineral *talcite*. Hence it will be perfectly proper, and perhaps necessary, to use the name *talcite schist*, or its congener, *talcose schist*. For these two reasons, then, we think it would be well to retain the old name for the present.

After discovering that magnesia in talcose schist was infrequent among our metamorphic rocks, the inquiry arose, How is it with talcose schist in other localities? Does it contain magnesia, or not? Mr. Barker referred us to four analyses from Bischoff (Bischoff's Geology, Cavendish Society ed. p. 352), three of which agree essentially with the Vermont specimens, but the fourth contained twenty-six per cent. of magnesia.

The non-magnesian specimens were from Sweden, Germany, and Tyrol; the magnesian example from Austria.

Bischoff adds this note: "What is often called *talcose* slate, contains none, or merely traces of magnesia, as analyses I., II. and III. show."

Thus, there are two great varieties of talcose schist in Europe, one of them corresponding to the aluminous schists in New England. Whether any true magnesian slate from this country has ever been analyzed, we are unable to say.

NOTE. — In a letter from Mr. T. S. Hunt, written since the reading of the preceding article, several suggestions are made which deserve to be mentioned, and therefore I add an extract from it.

"I did not hear your paper on talcose schists, and do not know but you have compared your results of the so-called magnesian slate, with a small amount of silica and much potassa, with the *dysyntribite* of Prof. Shepard; which forms beds in the Laurentian rocks of northern New York, and resemble serpentine. You will find analysis by Brush in Silliman's Journal, second series, Vol. XXVI. p. 68, as also analyses of what I called *parophite*. It is a shale of the Hudson river group, locally altered by trap, and changed into what seemed a serpentine, whence the name. In Brush's paper, analysis 3, p. 68, is of the unaltered earthy shale, which further on passes into a green, translucent, unctuous mineral — analyses 1 and 2. (See also my Report, as cited by Brush.) We can scarcely doubt that the mineral from the Laurentian of northern New York, where the same matter occurs crystallized as *gieseckite* (see Brush), is identical with your magnesian slate [the specimen from Pownal], and my parophite and unaltered shale; since they agree in their peculiar composition, so different from ordinary shales and slates. These matters I regard as perhaps a sort of kaolin which has fixed a portion of the potash."

I learn also, from Mr. Hunt, that he has found specimens of real talcose schist among the group of rocks the subject of this paper. Many investigations therefore will yet be needed to settle the question as to its name, satisfactorily.

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## 9. On the Marks of Ancient Glaciers on the Green Mountain Range in Massachusetts and Vermont. By Charles H. Hitchcock, of Amherst, Massachusetts.

THE discovery of the marks of ancient glaciers in Europe incited American geologists to explore prominent peaks and ranges of mountains in this country for similar phenomena. We believe that nothing has yet been published upon the occurrence of these marks, as distinguished from the drift or the *great glacier* 2,800 miles wide, and extending from the Arctic ocean to the 40th degree of north latitude, as some are pleased to call it, except a short communication to this Association at the Providence meeting, and a detailed account published in the Smithsonian Contributions to Knowledge, of certain marks of ancient glaciers in Massachusetts and Vermont, by Prof. Edward Hitchcock.

The cases already described are upon the Westfield river and its branches, and Deerfield river and its branches in Massachusetts, and upon the Otta Quechee river in Vermont.

Under Prof. Hitchcock's directions these investigations have been continued along the principal mountains west of Connecticut river in these States, and their results are briefly embodied in the present paper.

The following are the chief distinctions between the marks of the ordinary drift and the marks of glaciers:

1. Glacier striæ differ often widely in direction from drift striæ, and consequently their stoss sides were struck in quite different directions.

The drift striæ may be referred to three general directions in New England,—to the south, the south-east, and to the south-west. Hence, though the direction of the glacier striæ at some particular locality may be the same as that of drift striæ at a distance, they will not be confounded with drift striæ, because the drift striæ have a different direction in the vicinity of the glacier striæ.

So, also, a glacier may have occupied the same valley and have pursued the same course as the drift in north and south valleys. We do not pretend in such cases to say whether a glacier once existed or not.

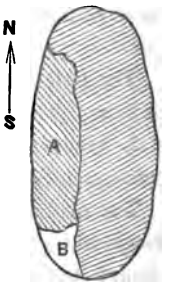
2. Glacier striæ occur only in valleys radiating outwardly from the crests of mountains; while the drift striæ overtop the mountains, and when found in valleys frequently cross them obliquely.

3. Glacier striæ descend from higher to lower levels except in limited spots where they may be horizontal. Drift striæ as frequently ascend mountains thousands of feet high, of which the lower is the stoss side.

4. Drift is spread promiscuously over the surface, and the blocks are a good deal rounded. The detritus of glaciers more or less blocks up the valleys or lies strewed along the sides of the valleys, and the fragments are frequently quite angular. These blocks, however, are sometimes covered more or less with materials that have been modified by water flowing for ages down the same valleys.

One of the most decisive cases of glacier markings is in the west part of Hancock, Vt., on the west side of the Green Mountains, and near their summit. It is in a valley through which Middlebury river commences to run, within half a mile of the crest of the Green Mountains, and nearly parallel with the range. Several ledges of the gneissoid rocks have two sets of striæ upon them — the one running S. 50° E., pointing towards the depressed portion of the mountain east, over which the turnpike runs to Hancock, with the stoss side on the north-west —

the other set pointing W. 30° S. down the valley, with the stoss side on the north-east.




The annexed figure will give some idea of the relation of the two sets of striæ to each other. The shape of one of the bosses of striated rock is given in the sketch, and the lines crossing it represent the striæ. Those upon the right hand side are the glacier striæ, running N. 70° E., occupying the stoss side of the ledge. But the lee side of the glacier striæ, *A*, is the stoss side for the drift striæ, whose direction is represented by lines tending S. 50° E. *B* represents a small part of the ledge that was the lee side for both sets of striæ, and is not marked at all. The length of the ledge is sixty-six feet, and its breadth forty-two feet. The altitude of the boss is eight feet.

Every ledge in the vicinity is thus striated, and we regard the cutting off of the one set by the other as very interesting. The glacier set are the most prominent of the two.

The two sets of striæ cross each other at an angle of 70°, or more properly speaking, their direction differs 110°, the force by which the first set was made pointing S. 50° E., and the other S. 60° W. or W. 30° S. The force by which the first set was produced was up hill towards the crest of the mountain, which a few miles north of the road we judge to rise 800 to 1,000 feet above the striæ. The force producing the second set or glacial striæ, was down hill along the course of a small stream.

Passing over the mountain towards the village of Hancock, we begin to descend on the east side of the range, passing among large blocks of stone, but at first the ledges are covered. As we enter the cleared land we find the striæ of another glacier in the bottom of the valley pointing N. 70° E., the upper and west side of the ledges being the stoss side, and therefore the agent of erosion was directed down the valley. In three places, also, before reaching the main branch of White river, we found accumulations of detritus stretching across a considerable part of the valley in the same manner as ancient terminal moraines are found in the valleys of the Alps. These have been somewhat modified by the subsequent action of water upon the surface, but their internal characters are the same as

those of moraines found near existing glaciers. At the village of Hancock the valley meets the valley of White river at right angles. The striæ are, also, at right angles, for another glacier descended the valley of White river; and, like modern glaciers, these ancient ones united their forces and travelled on together, a river of ice, bending its course to correspond with all the sinuosities of the valley, as the striæ indicate. The general direction of this valley is southerly, and hence does not differ greatly from the course of the drift in this vicinity.

We should expect that each tributary stream would have brought in its river of ice to unite with the main stream. We have examined but one of these tributaries, a stream running through the west part of Rochester to the east village of Rochester, and find there most distinct evidences of glacial action, more decided, in fact, than either of the other branches of White river, because the curves in this valley are much greater than in the others, and the striæ are found to conform to them with exactness. The map of the Surface Geology of Vermont, in the State Geological Report, now in preparation, will show the particulars of this statement.

The principal glacier continues to Stockbridge, near the great bend of White river, into which we were unable to trace the glacier. Probably its termination was near Stockbridge village, where an immense terminal moraine crosses the valley. The total length of this glacier, so far as investigated, is eighteen miles, and each of the tributaries examined is about four miles. The slope in all was gradual and uniform.

A less striking example of the traces of the action of ancient glaciers is found upon the Otta Quechee river, above Woodstock. In ascending that stream above that village we meet, within a mile or so, accumulations of detritus on the north bank, which we refer to moraines that have been subsequently considerably modified by water. Several miles further west is found a still more decided example just east of Bridgewater village. Here the detritus seems to have extended entirely across the valley, but has been worn away by the river on one side. Dr. Hitchcock refers to cases of extinct moraines in the vicinity of Chamouny, in Savoy, very similar to the foregoing.

In following up the south branch of Otta Quechee, which comes in from Plymouth, we find the west and south-west sides of the ledges rounded, though the striæ are mostly obliterated. On the more northerly branch of the river, at least as far as the gold mine in Bridgewa-

ter, where an auriferous quartz vein has been worked, the west side of the hills was found to be the stoss side, as it would be if a glacier once descended from the crests of the Green Mountains, near Killington Peak, down the valley of the Otta Quechee. And though this region has not been examined, yet we are inclined to believe that a system of glaciers once occupied the head valleys of the Otta Quechee, as of White river.

The position of the stoss side is of the utmost importance to one who would discriminate between these two classes of striæ. The stoss side of the drift is invariably upon some point of the compass north of an east and west line; whereas a glacier may strike a ledge from any direction whatsoever. Hence the importance of those stoss sides upon the Otta Quechee glacier, where the force came from the south and the south-west, in direct opposition to the drift currents.

*Other examples of Glacier markings.*— We have not time to describe other examples in detail, and will, therefore, simply mention their localities in Massachusetts.

Upon Deerfield river, near the proposed Hoosac Tunnel, one has been described by my father. Its upper part has been found recently in the town of Searsburg, Vermont. It passed through an exceedingly rough country, and in some places has completely removed all the soil from the rocks.

Upon Saxton's river, near its source in Windham, supposed glacial striæ were described at the Springfield meeting. Since then (Sept. 8) it has been traced several miles south-easterly, and most interesting cases of striæ, from the south to the north, were discovered at a turn in the course of the valley in Grafton.

There is, probably, another example upon a branch of Black river, at Mount Holly.

Upon a branch of West river, arising in Stratton and uniting with West river below the village of Jamaica, there are interesting traces of an extinct glacier. The valley for several miles is as crooked as the Rio Grande, with a general north-easterly direction. The markings conform to the curvatures of the valley.

In Huntington, on Huntington river, the striæ curve with a great bend in the valley, and vary in direction from N. 70° W. to N. 20° E.

Upon the west side of Mansfield Mountain, there is a *small glacier*, or *slide*, that occasionally leaves markings similar to those described. The slope of descent varies from 20° and 25° to 35°, and the direction of the scratches is N. 48° W.

Hence we see that probably the range of the Green Mountains was once covered with glaciers upon both sides, either before or since the period of the drift proper. For in all these cases, away from the valleys, the drift striæ were found in abundance, pursuing a course diagonally to or at right angles with the course of the glaciers. If, as is probable, the glaciers existed before the drift, it would not be strange to find that occasionally the traces of their existence had disappeared, because the icebergs, etc., had worn away the striæ, or confused the moraines.

We might expect, if we can show the existence of glaciers from southern Massachusetts to northern Vermont, that other high mountains to the east might be found with similar marks upon them. We have seen none in the north-eastern part of Vermont; but to be sure, on account of the inaccessibleness of that region, and the decomposing character of the rocks, and the vast amount of modified drift covering the surface, one might overlook many examples of glaciers. Most of the striæ discovered are recorded on the map, and its vacancy shows how rarely the striæ were found. Similar difficulties to the foregoing meet us among the White Mountains. Still we doubt not that careful research will bring to light examples of ancient glaciers among those majestic peaks, the monarchs of New England mountains and scenery.

There is a curious phenomenon of drift striæ in Vermont closely resembling the action of glaciers, with a single exception. There are three rivers that have cut through the highest part of the Green Mountain range, the Winooski, La Moille, and Missisquoi rivers. Upon the mountains and valleys between the rivers the striæ run about 10° east of south; but in the valleys of the streams the striæ follow the windings of the rivers through the range, in an easterly direction. They might be called glaciers *inverted;* i. e. if a glacier were to ascend a valley instead of descending, the phenomena would be those that occur upon these rivers. We explain it by supposing that some of the icebergs were deflected from their usual course by the valleys. When the water had fallen so much that the summits of Mansfield Mountain and

Camel's Hump peered above the sea, a small iceberg that had fairly entered the mouth of the valley, could move only in the valley. The water would be too low down to raise it over the sides of the valley, and as its rear is pressed by a current, it must move onward, and the opening through the hills is its only course.

The markings in these three valleys are the most beautiful and distinct of any drift phenomena we have ever seen. We could find no evidence of the former existence of a glacier in any of them.

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## 10. Lake Ramparts in Vermont. By Charles H. Hitchcock, of Amherst, Massachusetts.

Two years ago some of the newspapers described a walled lake in Iowa, and ascribed its formation to aborigines. At that time we made some counter statements, though not having seen them; yet presuming that certain agents at work in nature might perform the work without calling in the aid of man. Since then we have discovered similar walls of stone in Vermont, and venture to describe this form of drift under the name of *Lake Ramparts*.

We will describe the case in Iowa, as it seems to be the most perfect of all that have come under our notice. The lake is in Wright County, situated in a large plain, covering an area of about 1,900 acres, and is from two to twenty-five feet deep, with a hard, sandy bottom. Around the lake is a wall of heavy stone, in some places ten feet high, thirteen feet wide at the base, sloping up both sides to five feet wide at the top, and is entirely composed of boulders from fifty pounds to three tons in weight. In some places this rampart, with soil, forms a barrier preventing the meadows contiguous from overflow. The top of the wall is level, while the land is undulating, varying in height from two to ten feet for two miles in extent. The writer cautions his reader "not to imagine that this wall is as regular and nice as the wall around the fountain in front of the City Hall, in New York City," whence we conclude that there is a considerable irregularity to it. He supposed it to have been the work of aborigines.

We had hardly thought of finding these phenomena in Vermont, till our attention was called to a ridge of this description on the north shore of Willoughby lake. For forty or fifty rods the shore is fringed by a ridge about a rod wide, and five or six feet high, of coarse gravel and boulders. The outer side is perpendicular, and the inside slopes gradually towards the water, having the shape of a rampart in a fort. Rev. S. R. Hall, who was with us, stated that he had seen the same phenomena in other small lakes or ponds, and upon a map of the Surface Geology of Vermont he has marked six other cases; namely, on the north side both of Great and Little Averill ponds, in the town of Averill, in the north-east corner of the State; on the north shore of Island pond; the east shore of a pond in Maidstone; the east shore of Lake Memphremagog, at its south end; and on the east, west, and north shores of Franklin pond. Mr. Hall states that the rampart on this latter pond is fifteen feet high. We have ourself noticed a rampart of this kind upon Woodford pond, in southern Vermont, and have data from which we think it probable that another example may be found in Tinmouth.

These facts, we believe, are new to geology, and with these descriptions it is very evident why we have selected the name of rampart to designate them. But by what cause were they formed? Certainly not by human agency.

Some observations that were made years ago upon certain stones that appeared to move without any apparent cause, in Salisbury, Ct., Rhode Island, and elsewhere, and described in Vols. V., IX. and XIV. of The American Journal of Science, appear more closely connected with this subject than any thing else we know of.

In the cases mentioned, large stones were found to have been moved towards the shore of the pond, and by measurements the exact distance over which they were moved, in a single winter, for instance, was ascertained. One rock, weighing several tons, was removed three rods during one winter. Though several causes were proposed, none was so satisfactory as that which called in the expansive power of ice. If the ponds, or some parts of them, are rather shallow, and the bottom covered by boulders, the ice of the winter would enclose the stones, and the gravel of the bottom it may be, and, from its well-known property of expansion, it would force the fragments from the central parts of the

lake towards the shore. In one year the progress would be small, but in each succeeding winter the work would be resumed until the fragments would be driven to the shore; and as the level of the lake, or at least the ice, is usually higher in the winter than in the summer, they might be crowded a considerable distance beyond low-water mark, and in the course of ages the accumulation might be large, if the materials at the bottom of the pond were abundant. Formed in this manner, their outside would be as steep as rounded stones could be made to lie upon one another, and, as in the Iowa example and others that we have in mind, a levee might be formed, if there was a very shallow ridge in the middle of the pond. After the ridge was formed in the middle, the shallow part of the pond would dry up, and leave a meadow lower than the level of the water in the remaining pond, and protected from overflow by the levee. Hence its ascribed origin to man is not strange, when its practical value is discovered.

Similar cases of removal, by expansion by ice in winter, are common, especially in New England, as the farmers, who build strong stone fences in rather low ground by the side of ditches filled with water, find to their sorrow each spring.

We doubt not these cases are common; and now that attention has been directed towards them, we hope that they will be described, of more elegant proportions, than the meagre ones we have visited.

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11. ORNITHICHNITES. By ROSWELL FIELD, of Greenfield, Mass.

WHEN fossil foot-prints were first discovered in the sandstone of the Connecticut valley, it was indeed thought to be a great discovery; but that the tracks thus found were made by birds was received by men of scientific attainments with great distrust and skepticism. That they were tracks made by once living animals there could be no doubt, but that they were ornithichnites was very much doubted. And it was not until after my esteemed friend, Dr. Hitchcock, had spent much time in comparing, describing, and in distributing specimens, that the scientific public became satisfied that they were the tracks of once living birds. The great and only proof that they were the tracks of birds, is the or-

ganization of the fossil foot, in the number of their toes, and the number of joints, or labial expansions in the toes; in this they are supposed to agree, and probably do, with living types. This, with alternate steps of right and left foot, is all the evidence we have that they were the tracks of birds. Living in the immediate vicinity of Turner's Falls, the locality that has furnished the most numerous, and beyond all comparison the most beautiful specimens, my attention was drawn, years ago, to this particular subject, and it was from my farm that the late Dexter Marsh obtained his nicest specimens.

And it was in the vicinity of these falls where my much lamented friend, Dr. Deane, found "new walks in an old field;" where our most barren and rocky wastes were to him a garden of delight. It was here I witnessed their labors with pleasure, and, in a more obscure way, have followed in their footsteps.

I think I may safely say that I have uncovered more foot-prints and found more new species and a greater variety of tracks than any other man; I think I might also say, with propriety, than all others that have preceded me; and if I have learnt any thing on this subject, I have learnt it at the quarries. It is there, and there only, I have studied the history of Liassic days; and the more I have studied, the greater have been my doubts as to the ornithard character of any of the tracks which these tables of stone contain. I have seen thousands of tracks that others have not seen. With injudicious blasting and the carelessness of workmen, many nice specimens have been broken and lost; other slabs, literally covered with foot-prints, have been spoilt by sun-cracks, the stratum over which the animal moved being either too hard or too soft to receive or retain good impressions. All such are rejected and lost to the student at the quarries. I have no new theory to advance, and none to build up, but if I can rightly decipher these fossil inscriptions, impressed on the tombstones of a race of animals that have long since ceased to exist, they should all of them be classed as Reptilia.

If I have not studied this subject in vain, they were all quadrupedal. That they usually walked on two feet I admit, and that they could as readily walk on four when necessary, is equally true. In proof of this we find tracks as perfect as if made in plastic wax, which to all appearance, as to the number of toes and the labial expansions in the

toes, perfectly agree with those of living birds; and still we know that these fossil tracks were made by quadrupeds, by the impression made by their forward feet. And in other cases, where the animals sunk deep into the muddy stratum over which they moved, we know that they usually dragged their tails in the mud, leaving a groove ploughed up, from one half to an inch in width; this groove is not always found on the surface where the foot rested, the weight of the animal causing the foot to sink through the yielding stratum, whilst the tail dragged on the one above. And this we know was the case with animals that were surely quadrupeds; they show no such appendage as a tail only when the foot sunk deeply in the plastic clay, and the proof that we once relied upon to prove them the tracks of birds, can be relied upon no longer. The fact that there were quadrupeds in those sandstone days, which had hind feet perfectly agreeing with the stony bird tracks, throws great doubt and distrust on the opinion that there were any true birds in this age of reptiles. If there were birds, they were apterous and naked, for we should naturally suppose that, where so large a number of birds congregated upon the muddy banks, that in dressing and pluming their feathers some of them must have been trodden under foot; but the impressions of feathers have never been found, though we find the smallest leaves of vegetables, and the pathway and tracks of annellids and insects, some of them so small that they can hardly be seen with the naked eye. Even the Otozoum, whose giant-like track measures twenty inches in length, was once supposed to have been a biped reptile. Later discoveries have proved it to have been four-footed. And other new discoveries have reduced the number of birds and added largely to the quadrupeds; where I verily believe is the proper place to class them all. The smoothness of the bottom of the foot in our fossil tracks agrees better with some species of batrachians that now live in and about the water than they do with such animals as live on the land. Had birds indeed existed at this early geological period, when the sandstone of the Connecticut valley was depositing, there has indeed been a woful gap in their history from then up to near the historical period; and the die from which they were struck, at their creation, was not broken, but a new edition struck off in these latter days. The work perhaps may have been revised, but not enlarged; that is, as to the size of the animal. I know that many eminent men and men of great scientific attainments—men who have spent much time and labor for many years in the inves-

tigation of this subject, have come to different conclusions, and it is not for me to say that their conclusions are wrong; and I would only add, that when fossil tracks were first discovered, there was so little known of the formation of the feet of fossil or of living animals, and particularly of their foot-prints, that it is possible the first discoverers might have been mistaken as to their ornithard character. The study of these fossils is very interesting to the geologist and naturalist, and there is no locality in the known world where they are found in such an abundance, and beyond all comparison in such perfection, as at Turner's Falls, the northern terminus of the sandstone beds. Very few, indeed, have any conception of the marvellous perfection of these fossil inscriptions, as of the multitude of once living creatures, whose existence they commemorate. During the vast sandstone deposition countless individuals have inscribed upon the shores whereon they congregated their instructive history.

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## II. ETHNOLOGY.

### 1. The Indian Mode of Bestowing and Changing Names. By Lewis H. Morgan, of Rochester, N. Y.

Civilized and nomadic life have but few analogies. As conditions of society they are organized upon different principles, and governed by dissimilar motives. The civilized man and the nomad represent two marked varieties of the human race. Each appears to be living in obedience to certain primary ideas or impulses over which he has little, if any, control. Hence, as we should expect, their domestic institutions are in continued contrast.

An illustration of this will appear in the following brief explanation of the Indian mode of bestowing and changing names.

The Indian has no family name. His name is single, and, like the prænomen of the Roman, or our Christian name, is purely an individual designation. The *family*, in our sense, is wanting; so that the names of several brothers and sisters would not suggest the fact of any connection among them. His *tribe*, however, in a large and general sense, answered the same end and purpose the family does with us. It was

composed of a multitude of parts of families bound together as consanguinei under a common tribal name. As a tribe, also, it was a constituent part of the nation.

In bestowing and changing names their customs are original and novel. They have names adapted to different periods and pursuits of life: one class for infancy and childhood; another for manhood; another for their religious advisers, called by the Iroquois, "Keepers of the Faith," and another for chief and sachem. These names are not taken up and conferred at random, but under fixed regulations. Each tribe has its own tribal names, which are kept distinct, and which no other tribe is allowed to use. They are family names; for the tribe is but a great family of which the chief is the head. It is said by some of the Indian nations that these names have such tribal characteristics that the tribe of an individual may be known from his name alone.

To understand what follows, a word of explanation of these tribes will be necessary. Nearly all of our Indian nations are divided into tribes, sometimes called "clans," but this term does not well express the idea. These tribes are named after quadrupeds, birds, reptiles, and fishes, and sometimes after inanimate objects. The following are the names of some of the tribes now existing among our Indian nations, namely, the Wolf, Bear, Beaver, Turtle, Deer, Plover, Heron, Hawk, Eagle, Crane, Duck, Loon, Sturgeon, Carp, Eel, Pike, Cat-fish, Buffalo, Elk, Reindeer, Panther, Otter, Raccoon, Hare, Rabbit, Snake, Buzzard, Owl, and Pigeon. Also, Reed-Grass, Thunder, Sand, Water, Earth, Tent, and Rock. The ancient civilized nations named their tribes after men, as the Greeks and the Jews; so in modern times, in a more restricted sense, the Scottish clan and the Irish sept; but the Indian is never found by act or deed attempting to exalt the name of any man.

Of these tribes the Iroquois have eight, the Wyandottes eight, the Ojibwas seventeen, the Kaws fourteen, the Shawnees thirteen, the Delawares three, with subdivisions, the Mohekuneuks and Munsees the same, the Choctaws two, with subdivisions, the Winnebagoes eight, the Iowas eight, the Otoes nine, the Potawottomies fifteen, the Moqui of New Mexico nine, and in similar proportions in the other nations, except the Dakotas, who alone among all our Indian nations, where the inquiry has been made, deny that they have any tribal subdivisions.

The tribe is the unit or integer of Indian political society; and all of their institutions gravitate around this central idea. As to marriage,

no man is allowed to marry a woman of his own tribe. This was the ancient law in every nation without exception, where the tribe existed. They were consanguinei. The title of chief or sachem descended, and was hereditary in the tribe, although elective as between certain of the near relatives. The children follow the tribe of the mother, and belong to the mother in case of divorce, among the Iroquois, Wyandottes, Delawares, Mohekuneuks, Munsees, and Choctaws; the rule is the other way at present among the Iowas, Otoes, Pawnees, Omahas, Ojibwas, Otawas, Winnebagoes, and Kaws.

Upon the birth of a child, the mother or some relative in her behalf, applies to the chief of her tribe for a list of tribal names, which are not then in use, and of the class for children. Out of those named over to her she selects such a name as pleases her fancy, which is then agreed upon as the future name. At the next council the birth and name of the child, and the name of its father and mother are publicly announced; and this was the simple form of an Indian christening. Their names are significant, as all names originally were, but with the Indian their signification is still preserved. O-wí-go a "floating canoe," and Gä-hä-no, "hanging flower," are specimens of their childhood names.

At fourteen or sixteen years of age, the name became unsuitable, the person having reached maturity. A new name was then selected; and the taking away of the old name and the bestowal of the new, was made, in some of the nations, an important event. With this change he ceased to be a boy, and became a man. He could then go out upon the war path and speak in council. In some of the nations, as the Iowa, the Kaw, and the Otoe, the youth must go out upon the war path and earn a new name before he was allowed to change it; and if he performed some exploit of sufficient merit, his name was changed before his return.

The power to make this change was lodged primarily with the chief of the tribe. But it might be made either by the mother or by a brother or sister, but never by the father, and it was usually done without the consent or even knowledge of the person whose name was changed. If these near relatives neglect to make the change at the proper time, it then becomes the duty of the chief to do it. At the next public council the change and the new name are formally announced, and it takes effect only from the time of this announcement.

When a private person is raised to the dignity of chief or sachem, his former name is taken away, and a new name of the higher class is conferred in its place. This name can never afterwards be changed unless the sachem is deposed; for the name itself is a title, and with it would pass away the title or office itself. Neither can that class of persons who are called "Keepers of the Faith," among the Iroquois, change their names without giving up the office, as these names and the office they confer are inseparable. Any other person of mature years may change his own name, by his own motion, provided he can induce a chief of some other tribe than his own to announce the change in council.

Upon the death of any person, the right of giving his name belongs exclusively to the oldest surviving son or daughter. He or she might give it or withhold it altogether. The reason was that the immediate relatives of the deceased would be pained and perhaps annoyed at the assumption of their father or mother's name by another. They would be reminded of their loss with every repetition of the name; and it is one of their customs never to remind one of a bereavement by mentioning the name of a deceased relative. To appropriate a name, without consent, under such circumstances would be dishonorable, unless amends were made at the time by a present. Accordingly it has been an immemorial custom among the Iroquois "to cover the naked back," to use their own expression, "of the person whose father or mother's name had been appropriated without permission." The present, which was a dress of skins, was to the person to whom the bestowal of the name of right belonged. This made it an honorable transaction.

A tribe may lend one of their names to a person in another tribe, which is often done; but when this person dies or his name is changed, the name so borrowed returns again to the tribe.

New names are not now invented by the Indian any more than they now are by ourselves; but old names are handed down in the tribe as our names come down to us, but not shorn, as in our case, of their better part, their primary signification.

The usages of the Red Race in bestowing and changing names are not in themselves especially important; but in an ethnological sense, as one class of facts among several others of the same character, all tending to show the prevalence of the same ideas throughout the whole Indian family, they have an important bearing upon the question of the common origin of the race.

## III. POLITICAL ECONOMY.

### 1. On the Paradox of Political Economy in the Coexistence of Excessive Production and Excessive Population. By Clinton Roosevelt, of New York.

It was the idea of a certain ancient sect, that the gods created men to supply themselves with subjects of mirth in our absurd conceits. But in *ancient* times no such absurdity as distress from a superabundance of food and clothing in the midst of a crowded population, in want of every thing, ever was heard of.

And yet has it been admitted on the floor of the parliament of a nation boasting the most advanced civilization, that they know of no remedy for the starvation of the real producers of national wealth, in the midst of an excess of productions. One of her political economists, Malthus, a free trader, who holds that the supply of hands to labor ought to be made to keep pace with the demand, has proposed as a remedy for excessive production of goods, what is tantamount to infanticide.

This has been well characterized as the result of a "Juggernaut civilization." The wheels of overproducing machinery seeming to crush those who bow before them, saying, "Let men alone to buy cheap and sell dear, and things will regulate themselves."

But it appears from the paradox before us that

"Things are in the saddle, and ride mankind."

Things regulate themselves in the labor-saving factories by doubling down the ankles of the children of toil, and bending their limbs and bowing their backs to premature old age; and thus, with a thousand millions of man-power in machinery in Great Britain, more or less, it has been suggested by one of the learned doctors — Combe, of Edinburgh — that nineteen twentieths of the producing classes of Great Britain die prematurely, of diseases superinduced by excessive labor and deficient nourishment. For whenever they produce faster and more than those in better circumstances can consume or waste, they must stop labor, and wages can be no more received, until those who manage

the political economy of Great Britain and the world — so far as they do manage it — have made way with the excess of food and raiment and the luxuries of life. In the mean time, the Board of Red Cloth Almacks, the head of fashion, controlled by women, hold it as disgraceful for one in "good society" to sit at the same table with any man who earns his living by his honesty, industry, of hand and head. In *China*, it is said, the emperor goes out yearly to hold with his own hands the plough, to testify his honor to the arts by which all must live or die. Which is most civilized or christianized? But England often trembles for this sin as if balanced on an inverted pyramid, and earthquake throes were rocking it in the great masses which now underlie the heaving strata. This state of things is evidently most unnatural. Looking on the earth as one, and recognizing also this great truth, that all the interests of all mankind are by Providence connected in one bond of sympathy, so that even the weakest may not suffer without sending home to the great heart a pang of anguish; that must be indeed a very little, selfish heart who can say, "Let men alone to buy cheap and sell dear, and things will regulate themselves."

But besides the remedy of Malthus — infanticide — the British government have heretofore employed a standing army to shoot down "operatives," who, in times of excessive production must

"Beg a fellow worm to give them leave to toil,"

and yet find no employment, and thus compelled by *want*, which knows no law of God or man, commit a mad destruction of machinery, as if iron arms, and fire and water were conspiring to destroy them and their little ones, and no medium course were left but to destroy machinery or die. Hay and grain ricks, too, are fired, and cattle houghed, and all that man may have accumulated more than is enjoyed by naked savages, becomes the subject of evil eye of desperation. In the mean time, *we* go on improving in the arts and sciences. The great West is open to *our* crowded population, even when thrown upon our shores from the old world, and we rest content apparently. But none can be content. We may gather jewels of learning in the abstract sciences, but men are clamoring out of doors to us for something "practical." Shall we entirely disappoint their hopes?

We contend that these abstractions open up the way to higher good, aside from every petty party feeling. That if the habit of pursuing

truth for its own sake, does no more for a time than discipline the mind, there is only such a discipline required to open up the human understanding and the will to comprehend the cosmos and the universal science by which only may the general principles be understood, from which the world may yet escape the paradox, which brings to a dead lock all improvements in the arts of life, and drives back to barbarism, as the unfortunate result of *ignorance of general principles*.

But now to come down to the cause of the apparent paradox which forms the subject of this paper.

It would seem, to one who had not thought upon this subject, that nothing in nature can be easier than the cure of excessive production in the midst of an overcrowded, starving, and naked population. One has only to preach Christianity, and cry, "Come buy wine and oil; and he who hath no money, let him come buy wine and oil without money and without price."

It will be observed that the bone on which the misfitting shoe pinches is simply in the WANT OF MONEY on the part of those who want every thing else; that seems to be the great turning point of all the difficulty. I say *seems*, because the real difficulty lies deeper in human nature, since things physical when out of joint pain us because we are *metaphysically* wrong. But we will not at present pursue this point, since it is equally true that the physical reacts upon the metaphysical, so that the higher class of minds may well begin to "feed the hungry and clothe the naked, as the best means of making peace on earth and good will amongst men," — nothing being more true than the words of Bacon, that "of all causes of revolution, want is the worst." Moreover, statistics have shown, that in Scotland, after the improvement in looms took place, crime increased with the want induced by a continuous fall in wages, from between eighty and ninety cases per year, to between eight and nine hundred. More and more severe laws were enacted, but we repeat, "necessity knows no law." And as to Christianity, were the preachers to begin to cry as we have quoted, and thus invite the poor and the necessitous to take from granaries and storehouses "without money and without price," would they not themselves be sent up to the penitentiary, as inciters to mobs and riots, or to the lunatic asylum, as if madmen? The hallucination of kind hearts is powerless in cases which have taxed the intellects of calm, reasonable men.

Nor will a common stock of property avail. It has been often tried

The inducement to labor is destroyed when the indolent and careless folds his hands and throws upon the careful, prudent, and industrious the labors of the heat and burden of the day, and thus all fall apart unless sustained by some enthusiasm, which only in some few rare instances exists. And they must be sustained by laws of those who make the laws for them.

But to resume. It will appear that what both the people on one side, and the holders of large stocks of goods and produce want, in times of excessive production and excessive population — so termed — on the other, is more money to effect sales, and thus give employment and fair wages. The paradox is simply a stagnation in the circulating medium of commerce and trade.

As if the blood in a man's body had met with an obstruction by an ossification of the valves of the heart. When that happens, it is not because of any excess of blood, but because the blood has deposited bony matter where there ought to have been flesh, to speak generally.

I mean to say that the whole difficulty arises from the bad nature of the money of trade and commerce, whether it be specie or its representative, paper-money, or the representative of the representative, the credit system, on the books of bankers when no money actually passes. Our purpose is to make this truth appear.

But first let us refer to one or two facts, the results of experiments.

It has been discovered by experimental farmers, that by the spade husbandry Ireland alone is adequate to the support of all the people of Great Britain and Ireland. But beef, butter, hay, and potatoes have been exported from Ireland, while a large portion of the people were in actual want of the common necessaries of life. Their products were brought to America and sold at a profit to the merchant here, while it seemed *we* had an excess of productions. Thus the poor of that devoted country were starved from actual want, while our laborers were thrown out of employment.

It will be found, that in theory, the simple fact of the withdrawal of money from circulation in Ireland, and the issue of a greater amount of money — pure or mixed — in proportion to articles having exchangeable values, makes *prices lower* in Ireland than in this country, *entirely irrespective* of the crying wants of the poor producers of wealth in either country. In a word, *cheapness and dearness, or money prices, form no criterion by which to judge of the real value of any thing to any nation.* We require a true standard of national values.

Under present circumstances it is possible to bring to bear and concentrate a dozen of poor hands on a poor soil, with poor machinery, against *one* with every natural and artificial advantage. Indeed this is now actually done continually, and has been done for many years. Always, in fact, more or less, heretofore.

Take, for example, the cotton grower of Hindostan.

An *anna* — about three cents of our money — will pay for a day's labor, while a slave's labor in our southern States, valued in our depreciated currency, may be reckoned at thirty cents per day. It will be perceived that by this estimate the employer of free laborers in Hindostan is enabled to bring to bear ten hands against our one, and thus is it no longer a mystery that he can send his poor, yellow, dingy, short stapled cottons, on the backs of buffaloes a thousand miles, over bad roads, down to the coast; and thence around the Cape of Good Hope to Liverpool and Havre, and there sell it at half the price of our fine long staples — they having only the *chourka* to pick it from the seed, whereby it requires two men to pick out forty pounds a day, against our cotton gin, which cleans several hundred pounds a day by steam, or horse, or other animal power.

In a word, with every natural and artificial disadvantage, those "outside barbarians" are an overmatch for us, simply because it happens they have a currency small in proportion to all the means of subsistence, while we have a depreciated currency, in which much money is required to buy a little of goods or produce.

It will be perceived, that all free trade dogmas fall to the ground before these simple facts. As to tariff doctrines, they fare little better, because upon the principles just suggested, our bankers have only to emit a certain amount more of specie, paper, or credits, after an increase of the tariff of duties, and the foreign manufacturer finds prices in our country *raised so high* that he is thus enabled to pay our duties and still retain a higher profit than the American manufacturer can have, with wages and the raw material and usury enhanced against him, by the depreciation of our currency.

In truth, the foreign nation has only to withdraw a certain amount of its currency to counteract any rise we may make in our tariff. For these artificial causes of high and low prices are only one or two items in the calculations of the merchant, who even makes a profit on the duties he pays. All the merchant asks is "steadiness," so as to know

how to calculate. If you increase the currency, he imports, and gets the difference as profits on imports. If you decrease the currency, prices fall, and locked up granaries become profitable as furnishing materials for export. After the great crash in 1857, the granaries were unlocked, as the expansions of the currency were contracted, and in a few weeks balances against us began to be paid and confidence was restored. The writer can prove he foretold to a day, immediately after the failure of all the banks, when prices would begin to rise again; so entirely within the control of reason and science are these so-called "panics," which may well be considered as in fact the delirium tremens of trade, arising from that stimulant, bank paper, by the credit system.

But there are those who fancy paper-money has built up America, and made its "wilderness to blossom as the rose." That we could not move a step without it as a people, however almost every patriot from Washington down has warred against it, and Jefferson has termed the prejudice of some minds in its favor a *mania*. Indeed there are those who are inclined to doubt one's orthodoxy if he doubt the power of the holy ghost of specie, "the almighty paper dollar." It is no part of our duty to reason with such madmen. We refer them to authorities. Webster has said truly, "The founders of our government, and those who enacted the first statutes on the subject, were *hard money men*."* But considered scientifically, the evils of "*hard money*" are almost as great as those of the ordinary kinds of paper-money. This will be perceived on an analysis of the regular course of trade, in any specie-paying country. Let a certain district represent the world, and this fact will appear. Seeming excessive production will arise and its concomitant, excessive population. Thus:

Remembering the fact that a given amount of money represents a given amount of goods by prices dependent on the amount of labor it would cost to produce the like as a general rule, prices must fall as the amount of goods increases by machinery, and this destroys the inducement for the master manufacturer to go on employing hands, because thereby he cuts short his profits and must fail in business. The prices at length would not pay him for the labor and the raw material. The effect is the same as a curtailment of loans and discounts by the banks. For it is just the same in effect if we make more goods in proportion

* In his great argument on the charter of the United States Bank.

to the amount of money in circulation as to make less money in circulation in proportion to the amount of goods manufactured and on sale.

I have stated thus the whole difficulty in the paradox termed excessive production and excessive population. The money to represent new goods does not increase side by side with the increase of goods manufactured.

It may be thought that paper-money fabrications come in exactly to supply this want. But such is not the case. They do business only to make good dividends, and "let things regulate themselves." They do not take these matters into their accounts at all. Mr. Biddle admitted that the disorders in the currency, and trade and commerce arose from "overtrading, brought on by overbanking, and the remedy was to bank less and trade less," but we find him afterwards buying up all the cotton and inflating prices until the lion in the Bank of England — that power behind the throne, and greater than the throne — set his paw upon him by declaring he would discount no more paper of any in the American trade, whereon our staples fell one half in price, our exchange came back with twenty per cent. damages against us, whole rows of buildings in our country, with our New York Exchange, fell into British hands, and when we had left upon our backs hardly a last garment, that priest of Mammon, the Rev. Sidney Smith, who laid up his treasures in Pennsylvania bonds rather than in heaven, raised the cry of stop thief on us all over Europe for repudiation. Such financial forays may be practised on us and are practised, so that as a nation we are swamped in debt, with hardly the power to sustain a three years' war.* Indeed at that time Mr. Webster said the British should be driven from the Aroostook territory before the Fourth of July, but all our money being drawn away, the "sinews of war were cut," and he accepted as a fair remuneration for a military road to our country's heart — the great lakes on our northern frontier — a penny whistle in the shape of a poor fort, in our power at any time, and some other trifles, and when asked why he had not driven the British off as he had said, replied he "did not say what Fourth of July he meant." In a word the depreciated currency of our country induces only a superfi-

* The first act of England is to break every bank, and every bank then turns against our government and charges enormous usury for loans of paper, so that we are robbed more by the banks than by the open enemy. Our government securities fell ten per cent. a year in our last English war.

cial appearance of prosperity, and only weakness in the trying hour. It renders us an importing nation — we sink in debt when we have every natural advantage to export. In the mean time what ought to be regular business is more hazardous than a fair game of chance.

But to some this is the greatest recommendation of the credit system. Hear what Voltaire confesses for himself upon this subject as innocently as any "babe of grace." "Here am I," said he, writing to a friend from Ferney in Switzerland, "living in a way suited to my habits and caring little for to-morrow, for I have a friend a director in the Bank of France who writes to me whenever money is to be made in the public funds. Sometimes he writes to me desiring me to sell because we are going to withdraw a quantity of notes; at other times he bids me to buy because we are going to issue more notes, and so through the kindness of my friend I always make money."

But this is evidently no better in principle than to play in a faro bank with marked cards, or at any time with loaded dice.

In ordinary gambling the banker lets his customers know exactly what percentage of the game is in his favor, but on the Voltarian system of expansions and contractions of the currency in our country, it has been calculated that ninety-seven per cent. of all who deal with our bankers fail. Indeed many who act without knowing the nature of the game which larger banks may play on smaller, as in the Biddle case and Bank of England, are themselves the victims. The great fish, in a word, devour the small. But to explain these things is not a certain cure of them. Many in like manner use tobacco and also ardent spirits, knowing they do them injury. The difficulty, as before hinted, lies far back in human nature, and the constitution of society. For the human will is governed as the mariner's needle is operated on by loadstone. A small fragment of loadstone, near the needle of the compass of the mariner, deflects it from the northerly direction more than the large mass remaining at a distance, and hence is it that many actually believing in eternal rewards and punishments, still give way to the present momentary temptation, regardless of all principles, until death approaches and stares them in the face, and then it is too late.

The analogy of physical science is found thus in the moral and religious. Motives operate upon the human will — taking time instead of space — inversely, as the squares of the distance.

Thirty-nine lashes directly laid on the bare back of a thief has a more

moving effect on him to the path of common honesty, than all the moral suasion in the world. Only let him be sure to take the remedy the moment he is caught, and the argument comes home at once to his business and bosom. In a word, the animal man, without moral sense, and holding to the principle that "might or cunning makes all right," can be reformed only on his own principle. With what measure they mete to others must it be measured unto them again. Moral suasion can have no effect where moral sense is wanting. Such are the conclusions to which this most important subject leads. What is required as far better than punishments, however, is a system of coöperation and a medium of exchange, by which it shall be to the direct self-interest of each to act consistently with the greatest good of all, as in an army in action every blow the soldier strikes and every shot he fires is as much to the advantage of him as to the whole. In the mean time each has his own pay and spoils to dispose of as may best suit himself, without doing injury to others. And this is the grand desideratum of scientific government.

For the common idea of ruling by punishments exclusively, presupposes the rulers are better than the people, whereas the experience of the world proves precisely the reverse. The lambs of the flocks are always in more danger from the shepherds who love succulent young flesh, than from the wolves. At least it is only a question of time which shall devour them. Society is like a perpetual motion without a prime mover, and it does not go. It comes to a dead lock in ways less innocent than we have seen in the explanation of the paradox which was the more immediate subject of this paper.

It seems to the writer, therefore, that the calm reasonable minds which constitute this learned body of scientific men, while they have done well to bring in the mathematicians and the chemists, know full well — being founded by geologists — that the world not having been "made in a day" ought to go on and establish an *ontological department*, for the discussion and establishment of general principles of political economy and government, including thus "all that man desires on earth as useful or delightful to him." Society at large calls for such a demonstration at our hands. The steps taken hitherto lead to it. The men who can take the lead in chemistry, geology, and the mathematics have compeers in other sciences and arts, however excellent themselves. Why, therefore, not extend to them the welcome hand of confraternity, in a country where our interests are one and indivisible?

# TITLES

OF

# COMMUNICATIONS.*

## A. MATHEMATICS AND PHYSICS.

1. On the Theory of the Comet's Tail. By Benjamin Peirce.
2. On the Mass of the Moon. By Stephen Alexander.
3. Some Observations on Ozone. By John Brocklesby.
4. A Question on the Earth's Dimensions and the Metre. By Stephen Alexander.
5. On the Mathematical Theory of Music. By T. H. Safford.
6. On Meteorology. By Joseph Henry.
7. Researches on the Platinum Metals. By Wolcott Gibbs.
8. Frozen Wells and Cold Springs. By John Brocklesby.
9. Note on Comets. By Stephen Alexander.
10. History of the Theory of the Physical Constitution of Comets. By Benjamin Peirce.

* The following papers were also read: of some, no copy has been received for publication; of others, it was voted that the title only should be printed. No notice, even by title, is taken of articles *not approved*.

11. On the Alleged Occurrence of Sand in Maple Sugar. By E. N. Horsford.

12. On the Source of Carbonate of Lime in Organic Structures occurring in Sea-Water. By E. N. Horsford.

13. Harmonies and the Ancient History of the Solar System. By Stephen Alexander.

14. Personal Peculiarities of Astronomical Observers. By Benjamin Peirce.

15. On the Determination of a Comet's Orbit. By T. H. Safford.

16. On the Geometrical Construction of Curves of higher degrees than the second, having given multiple points. By H. A. Newton.

17. Hindu Astronomy. By W. D. Whitney.

18. Common Origin of the Asteroids. By Stephen Alexander.

19. Disappearance of Ice in Northern Lakes. By J. G. Totten.

20. Heating Power of Flames. By G. A. Gould.

21. Areas of Closed Curves. By George Eastwood.

22. On the Prime Seventh as an Essential Element in the Musical Canon. By H. W. Poole.

23. Rains and Winds of Western Coast of United States. By A. D. Bache.

24. Carbonic Acid in Congress Spring. By W. E. Hughes.

25. Prevention of Fermentation of Juices of Fruits by means of Sulphite of Lime. By E. N. Horsford.

26. Causes of Changes in the Earth's Temperature. By Benjamin Peirce.

27. On Mortars and Cements. By Q. A. Gilmore.

28. The Cause of Steam-Boiler Explosions. By James Hyatt.

## B. NATURAL HISTORY.

29. On the Occurrence of Pot-Holes formed by the Drift Agency. By Oliver Marcy.

30. On a Frozen Deposit of Modified Drift. By Edward Hitchcock.

31. On the Conglomerate near Newport, R. I., with elongated pebbles and transverse joints. By Edward Hitchcock.

32. On the Occurrence of Bones and Teeth in the lead-bearing Crevices of the North-west. By J. D. Whitney.

33. On the Geological Position of the Bones of the Extinct Peccary of the West. By J. W. Foster.

34. Is Indian Corn (*Zea Mays*) a Native of three Continents, like Cotton and Tobacco? By J. H. Gibbon.

35. On the Amount and Proofs of Erosion in Vermont, with Special Reference to the Peaks of Protrusive Rocks. By Edward Hitchcock.

36. A Systematic Reference Catalogue of all the described North American Lepidoptera. By John G. Morris.

37. On Apparent Equivocal Generation. By H. J. Clark.

38. On the Lasso-Cells of Polypi and Acalephæ. By H. J. Clark.

39. On a Deposit of Fossiliferous Limestone beneath Granite and Mica-Slate, in Derby, Vermont. By Edward Hitchcock.

40. On the Scope and Method of Linguistic Science. By W. D. Whitney.

41. Certain Arts which distinguish Nations of the Eastern World from the Aborigines of the Western Continent. By J. H. Gibbon.

42. System of Consanguinity of the Red Race and its Relations to Ethnology. By Lewis H. Morgan.

43. An Attempt to prove that the Younger Metamorphic Rocks have been in a Plastic or Semi-Plastic State since their Original Consolidation. By Edward Hitchcock.

44. Theoretical Explanation of the Similarity between the Flora of North-eastern Asia, and that of Eastern North America. By Asa Gray.

45. On the Faceted Eyes of Acalephæ, especially of Aurelia flavidula. By H. J. Clark.

46. Remarks on the Fossil Whale of Vermont (with an exhibition of the skeleton). By Edward Hitchcock, jr.

47. Supposed Meteorite from North Carolina. By C. U. Shepard.

48. Supposed Shooting-Star which fell at Charleston, S. C. By C. U. Shepard.

49. The Placer Gold Mines of Georgia, and the Introduction of Improved Methods of Working them. By W. P. Blake.

50. On the Origin and Formation of Siliceous Rocks. By T. S. Hunt.

51. The Relations of the Upper Carboniferous Rocks of Illinois to the Older Members of the Palæozoic System. By J. H. M'Chesney.

52. On the Stratographical Position of the Sandstone of the Connecticut River Valley. By J. D. Whitney.

53. On the Formation of Gypsum and Magnesian Rocks. By T. S. Hunt.

54. Certain Arts which distinguish Nations of the Eastern World from the Aborigines of this Western Continent. By J. H. Gibbon.

55. On a Remarkable Vein of Gold in the Bed of the Chestatee River, Georgia. By W. P. Blake.

56. On the Formation of Continents and Oceans. By Joseph Leconte.

57. Vital Observations and Statistics. By E. B. Elliot.

58. On the Vertical Planes in Bituminous and other Coals. By E. B. Andrews.*

59. On the Minerals and Ancient Mines of the Cherokee Valley River, North Carolina. By W. P. Blake.

* The object of this paper was to invite the attention of observers to the fact, that these vertical and crystalline planes have, over immense areas, *a uniform direction*, and thus to secure data for future generalizations in regard to the nature and direction of crystalline forces, in palæozoic times.

# EXECUTIVE PROCEEDINGS

OF THE

# SPRINGFIELD MEETING, 1859.

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## HISTORY OF THE MEETING.

THE Thirteenth Meeting of the American Association for the Advancement of Science was held at Springfield, Mass., commencing on Wednesday, August 3, and continuing to Tuesday night, August 9.

The number of names registered in the book of members in attendance on this meeting is *one hundred and ninety*. *Eighty-five* new members were chosen, of whom all but *twenty-one* have already accepted their appointment by paying the assessment, and *fifty-two* of them signing the constitution. *Thirty-four* others joined the Association by virtue of the second or third Rule of the Constitution, all of them signing the constitution and paying the assessment. *Three* others paid, without a formal election, or signing the constitution; and *one* signed the constitution without a formal election, and without paying. One hundred and nine papers were presented, of which most were read; but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by the authors.

The sessions of the Association were held in the rooms of the City Hall.

The meeting was called to order by Professor Stephen Alexander, President of the Springfield meeting, who requested Rev. George B. Ide, D. D., to open the services with prayer.

Professor Alexander then addressed the convention as follows:

FELLOW MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: — Entertaining, as I believe I do, no other than a just estimate of all that would be appropriate in your presiding officer, you will pardon me when I say that it is with an unfeigned self-distrust that I enter upon the discharge of the duties to which you have invited me, as being those of a position which I would not have sought, but which — regarding it as a testimonial at once of your confidence and your kindness — I have yet felt to be one which I might not, ought not, to shun.

In the exercise of an earnest reciprocal feeling, relying implicitly on that same kindness and confidence, I shall offer no apology in advance for the manner in which I may discharge the duties of the chair; though to the fullest extent sensible of how often I may need your indulgence.

The American Association for the Advancement of Science, invited and encouraged though its members were by all that had preceded it, was yet entered upon with judicious caution, as an experiment; but it has now become an enlarged and progressive experience. Eminently worthy are the objects which it contemplates, and worthily should they be pursued, with a unity of feeling and of purpose, akin in sincerity and earnestness to that experienced by children who, hand in hand and shoulder to shoulder, are perusing — in our case, even reverently perusing — the pages of the same book.

Nay, more; this oneness of feeling, of purpose, and of action, too, is even vital as it respects efficient progress at all. As in the human system, the nerve of sensibility is throughout associated and intimately connected with that of voluntary motion, so that either an inflammation or a paralysis of the one would almost of course be fatal to all that is either durable or useful in the other; and as, withal, conversely, an appropriate activity is itself indicative of a quick-set vitality, so the origin and veritable progress of an associated body, such as this, are themselves the index and the measure of a sympathetic coöperation.

While, in the good providence of God, so many of us are permitted to reassemble here to-day, some, even since our last meeting, have ceased to be numbered with the living; and first among them, in the order of time, was one of our most venerable members; one whose devotion to the great object of our association began ere science in this

country had fully asserted itself, and coöperation, such as we now have it, was comparatively unknown. Yet no history of chemistry at least could be regarded as complete which should fail to render due honor to the genius and early labors of Dr. Robert Hare. But not alone chemistry — astronomy, too, has had cause to mourn — to mourn that he whom she has lost was cut off ere he had attained to an age which would entitle him to the epithet of venerable. Though frequently threatened with dissolution, it is yet in an important sense true, that up to the limit when he ceased to be seen among living men, the eye of William Cranch Bond was not dim, nor his natural force abated. The memory of his ingenuity and his discoveries furnishes him with an epitaph always readable and always read.

Almost at the moment of our meeting, the progress of the anniversary exercises of Dartmouth College has brought to mind the loss which that institution has sustained in the decease of Prof. Ira Young, who was elected at our seventh meeting, and has at other times been present and assisted in our deliberations.

And oh! we miss, withal, the cordial grasp and friendly greeting of Prof. Denison Olmsted, best esteemed and loved by those who knew him best. We must not be betrayed into his eulogy, which it belongs not to us to utter; yet assembled as we are at this very season — even here and now — we might well be reminded that the progressive movement toward a regular and extended observation of the periodic meteors, that this derived from him its projectile force. In our annual catalogue of members the fatal asterisk is prefixed to yet two other names, — that of Joseph Clark, Esq., of Cincinnati, elected at our fifth meeting, and Lieut. R. Ap Catesby Jones, of Washington, elected at our eighth meeting. The removal of the dead imposes new obligations to a united and vigorous effort on the part of those who survive.

Under every aspect, we have reason to congratulate one another on our place of meeting. What wonder that it needed no second invitation to induce us to select such a place as this; while we at the same time remembered, as we ought, how Springfield invited us three years ago. And even now, arriving as we do in obedience to our own motion, we find that the citizens of Springfield, with a courtesy and a kindness which are their own best praise, stand ready to welcome, in the person of their most appropriate representative; — a welcome from which I will no longer detain you.

Mayor Calhoun was then introduced to the meeting, and welcomed the Association to the city in the following address:

MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, — In behalf of the government and citizens of Springfield, I have the honor and pleasure of bidding you welcome on this, the occasion of your annual meeting. The invitation to pass the week, which you devote as a body to the claims of Science, in this comparative retirement in the valley of the Connecticut, was presented to you by the public councils of this city of a former year. They honored themselves by thus honoring you, and, through you, the cause of science. The present city government respond fully and cordially to the wishes, the purposes, and the salutations of their predecessors.

We bid you welcome, because we know, and see, and feel the importance of science, and of all good learning and knowledge. It is true, we have no institution amongst us devoted distinctly and systematically to the promotion of science, aside from those schools in which, throughout all the accustomed gradations, our youth are trained to that mental culture which our laws so happily require. But we have here, very largely, the fruits, the practical applications of science — in that large body of intelligent mechanics and artisans of various classes, who, if you shall have leisure to examine their works, will be reminded by you of the great principles developed by yourselves and your coadjutors, on which their beautiful and finished products depend.

We welcome you, because we desire to seize this occasion, and all occasions, to renew our fealty to the great purposes of the men to whom we owe our origin. They placed religion first — blessed be their memory that they did so. But they made all science, all knowledge, the object of their unceasing devotion and unwearied efforts. We hail the advent of this Association amongst us, because we desire to be drawn off from schemes and exhibitions, merely or mainly of a sensual and material kind, to such as make man, the immortal spirit, prominent and preëminent. We are reminded, too, of the obligations devolving upon us by the provisions of our constitutional charter, which declares it to be a "duty" "to cherish the interests of literature and the sciences, to encourage private societies and public institutions; to promote agricul-

ture, arts, sciences, commerce, manufactures, and a natural history of the country; to countenance and inculcate the principles of humanity and general benevolence, public and private charity, industry and frugality, honesty and punctuality, sincerity, good humor, and all social affections and generous sentiments among the people." I need not say how intimately and indissolubly these noble requirements of the constitution of our Commonwealth stand connected with the pursuits in which the members of this Association are engaged.

We welcome you to this portion of our country, gentlemen, because we think you will here find some things to interest you in the walks of science, strictly so called. You will visit the college in our vicinity — a rising and already distinguished institution of learning — in whose halls you will be greeted by the voice of science, and be privileged to witness its large and increasing appliances, and to rejoice in some of its triumphs. Here, too, in this range of country, you will find those tracks of birds and animals, various and numerous, which have attracted so much both of popular and scientific attention, and which carry us back, in a sort of bewilderment, to ages, if not beyond the power, at least beyond the certainty, of computation; those tracks which have been made to utter the language of science through the researches and investigations, as modest as they have been profound, of an eminent associate of your own.

We welcome you, because we delight to do honor to those pursuits which, in retirement and repose, without any of that glare which too often blinds and bedizens, are working out the true good of man, and garnering treasures of unfading renown. We are just now beginning to breathe freely again, after the monstrous exhibitions of self-glory, treachery, and war on the plains of Italy. Whilst, under the pressure of power and carnage, man continues to be trodden under foot, and the world shouts victory, be it ours still to strive and pray, that the world may, even though slowly, yet surely, be advancing in wisdom; and to rejoice in the progress of that science which elevates, never depresses, humanity.

We welcome you, gentlemen, in a closing word, because of the influences which we cherish the hope, nay, the unfaltering belief, you will be sure to leave behind you — influences to reach the heart, to be felt in the life, and to lift up the spirit: influences which may

guide us to the meaning of the poet, when he answers his own question,

"What constitutes a State?"

by that apt and terse averment,

"Men, high-minded men."

These constitute a State. These are at once the ornament of a community, and its imperishable foundation.

Men of science, we greet you; we bid you welcome.

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On Friday, August 5, at 8 P. M., the address of the retiring President was given at Hampden Hall, by Professor Alexis Caswell.

No lengthened abstract of the proceedings, scientific and executive, of the Baltimore Meeting of the Association, is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume.

The Association voted to hold their next meeting at Newport, R. I., on Wednesday, the 1st of August, 1860.

The officers elected for the next meeting are, ISAAC LEA, Esq., of Philadelphia, Penn., President; Dr. B. A. GOULD, Jr., of Cambridge, Mass., Vice-President; Professor JOSEPH LECONTE, of Columbia, S. C., General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, was elected at Baltimore for a third term, commencing with the Springfield Meeting.

During the session of the Association, the members and their ladies, besides receiving many private hospitalities, were elegantly entertained on Wednesday evening, August 3, by Hon. George Bliss; and on Thursday evening, August 4, by the Ladies of Springfield, at the City Hall. On Saturday afternoon, an excursion was made, by invitation, to Amherst College, to witness the rich scientific collections of that institution. On this occasion the students escorted the Association and their ladies, including many ladies and gentlemen of Springfield, from the depot to the beautiful hill on which the Colleges stand, and an excellent collation was provided by the ladies of Amherst.

## RESOLUTIONS ADOPTED.

*Resolved*, That the American Association for the Advancement of Science regard with great interest the efforts making by the British Association for the Promotion of Science, to induce the reëstablishment of the Colonial Magnetic Observatories for a new series of simultaneous magnetic and meteorological observations.

*Resolved*, That the officers of this Association be requested to communicate this resolution to the officers of the British Association.

*Resolved*, That the Association commend to the friends of Astronomical Science throughout the country the suggestion of Lieut. J. M. Gilliss, that another expedition be undertaken to determine the solar parallax, by observations of Mars and Venus.

*Resolved*, That a committee of seven be appointed by the President of the Association, to consult with Lieut. Gilliss in relation to the means of carrying out the suggestion in the most effective manner.

*Resolved*, That the Permanent Secretary be requested to notify the chairman of each special committee, one month before the annual meeting, that a report is expected from him.

*Resolved*, That the thanks of the Association be presented to Professor Caswell for his valuable address as retiring President, and that 250 extra copies of it be printed for his use.

*Resolved*, That Prof. Joseph Leidy be requested to address the Association upon the extinct Mammalia and Reptilia of North America.

*Resolved*, That Prof. Joseph Henry be requested to deliver a lecture on Meteorology before the Association at the next annual meeting.

*Resolved*, That Prof. A. D. Bache be requested to deliver a lecture on the Gulf Stream at the next annual meeting.

*Resolved*, That Prof. Joseph Henry be requested to prepare a memoir to be read before the American Association, on the scientific services and contributions of the late Dr. Robert Hare.

*Resolved*, That no action is necessary in regard to the motion to admit ladies as members, inasmuch as two ladies have already been admitted.

*Resolved*, That it be recommended to the Association of the next year to hold a winter session, in some southern city.

## VOTES OF THANKS.

*Resolved*, That the thanks of the Association be presented to the members of the Local Committee, their distinguished Chairman and efficient Secretary, for the valuable services which they have rendered us in providing every possible facility for carrying out the objects of our meeting.

*Resolved*, That the thanks of the Association be presented to the Mayor and Corporation of the city of Springfield, for the courteous welcome extended to us, and for their liberality in appropriating to our use the rooms of their fine City Hall.

*Resolved*, That we present to the ladies of Springfield our most cordial thanks for the brilliant and most successful reception given to the ladies and gentlemen of the Association, at the City Hall.

*Resolved*, That the thanks of the Association be presented to the Faculty of Amherst College, for the courteous invitation to visit them, and for the liberal manner in which they threw open to us their Scientific Treasures. Also to the ladies of the beautiful village, for their elegant entertainment; and also to the Superintendents of the Railroads for their liberality and good management in the appointments of the excursion.

*Resolved*, That the thanks of the Association be presented to the Superintendent of the United States Armory, and to the directors and proprietors of the other institutions of public interest, who have tendered to us courteous invitations to visit them.

*Resolved*, That the thanks of the Association be tendered to the Railroad Companies and Steamboat Companies who have shown their interest in the advancement of science, by extending to its cultivators gratuitous facilities for returning from this meeting, and whose names will be found in the circular and card of the Local Committee, as well as be practically made known to the members from a distance.

*Resolved*, That the thanks of the Association be presented to its President for the courtesy and impartiality with which he has discharged the duties of his office; and to the General Secretary, for the precision and system which have so much facilitated the despatch of our business.

## REPORT OF THE PERMANENT SECRETARY.

The Constitution of the Association requires that the Permanent Secretary of the Association should make an annual report, to the Standing Committee, of the business of which he has had charge since the last meeting of the Association.

The official duties of the Permanent Secretary, which he has been called upon to discharge since the meeting at Baltimore, may be classified as follows:

1. Correspondence for the Association, which is vast and laborious.

2. The issue to about one thousand individuals of various circulars and notices.

3. The collection of the annual assessment from so large a number.

4. The preparation of the Baltimore volume of Proceedings for the press, its revision, publication, and distribution, and the sale and distribution of the twelve volumes of Proceedings generally, at *home* and *abroad*.

5. The care of the property of the Association; namely, its manuscript Records and its printed Proceedings — the latter of which now amount to five thousand five hundred and eighty-two (5,582) volumes, which are carefully cased and deposited in a separate room at Cambridge, for which the Association pays no rent.

The number of living members of the Association at this date (August 3, 1859) is nine hundred and sixty-two (962); sixty-six (66) names have been struck from the previous list, each of whom was in arrears for nine dollars, making a total repudiation of five hundred and ninety-four dollars (594); and ninety-five (95) new members were added at Baltimore, so that the net increase above the number printed in the Montreal volume amounts to twenty-nine, and of living members to twenty.

From the commencement of the Baltimore Meeting to this date (the morning of the first day of the Springfield Meeting), eighteen hundred and ninety-six dollars and thirty-three cents ($1,896.33) have been collected by the Permanent Secretary; seventeen hundred and sixty-three ($1,763) in payment of assessments, and about one hundred and thirty-three dollars by the sale of volumes of Proceedings. The arrears now

due from old members, exclusive of the Springfield assessment, make an aggregate of two thousand one hundred and nine dollars ($2,109).

The expenditures incurred for the Association during the same period have drawn from its treasury sixteen hundred and sixty-one dollars and seven cents ($1,661.07), the details of which are printed in full in the Springfield volume. They may be distributed as follows:

1. For printing, binding, illustrating, etc., the Baltimore volume, nine hundred and ten dollars.

2. Expenses in connection with the Baltimore Meeting, one hundred and eleven dollars.

3. Salary of the Permanent Secretary, five hundred dollars.

4. Postage, stationery, etc., one hundred and forty-one dollars nearly.

The balance in the hands of the Permanent Secretary has increased since the last annual report from three hundred and fifty-three dollars and fifty-nine cents to five hundred and eighty-eight dollars and eighty-five cents. It appears by the Treasurer's report that the balance in his hands has increased also, by six dollars, and that it now amounts to five hundred and twenty seven dollars and seventy cents. This balance suffices to keep the Association sufficiently beforehand in funds to enable it to make its contracts for paper, printing, etc., to good advantage. It could not safely be diminished; and is not likely to be much increased.

JOSEPH LOVERING,
*Permanent Secretary.*

SPRINGFIELD, August 3, 1859.

## CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION *in*

| | |
|---|---|
| McDonald, for binding records, | $8.00 |
| Express to Baltimore, | 2.24 |
| Selby, as clerk at Baltimore, | 19.00 |
| Waitt, travelling box for records, etc., | 2.00 |
| J. D. Runkle, services on programmes, | 15.00 |
| Metcalf & Co., for circulars, | 9.50 |
| Buck's express, | 26.24 |
| Mills, for services, | 20.00 |
| Hollingworth for old volumes, | 2.25 |
| Glover for paper, | 243.05 |
| Bien, for printing plates, | 106.50 |
| Kilburn & Mallory, for wood-cuts, | 24.50 |
| Allen & Farnham, for printing, | 428.88 |
| Rice & Kendall, for covers, | 6.42 |
| Elliot's bill for committees, | 3.00 |
| Higgins & Bradley, for binding, | 68.18 |
| Salary of Permanent Secretary, | 500.00 |
| Expenses in attending meeting, | 75.00 |
| Welch & Bigelow, for circulars, | 10.00 |
| Postage and discount, | 43.70 |
| Index to Baltimore volume, | 5.00 |
| Stationery, | 10.00 |
| McDonald, for binding, | 1.50 |
| Journey to Springfield in May, | 16.00 |
| Buck's express, | 15.11 |
| | $1,661.07 |
| Balance to next account, | 588.85 |
| | $2,249.92 |

## PERMANENT SECRETARY.

*Account with* JOSEPH LOVERING. Cr.

| | |
|---|---|
| Balance from last account, . . . . . | $353.59 |
| Assessments, etc. (from No. 1 to No. 476 of Cash Book), | 1,896.33 |
| | $2,249.92 |

## STOCK ACCOUNT OF THE PERMANENT SECRETARY.

*Volumes Distributed or Sold.*

| Volumes | I. | II. | III. | IV. | V. | VI. | VII. | VIII. | IX. | X. | XI. | XII. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| DELIVERED TO | | | | | | | | | | | | |
| Boston City Library | | | | | | | | | | | | * |
| Nantucket Athenæum † | | | | | | | | | | | | * |
| Albany State Library | | | | | | | | | | | | * |
| Phil. Acad. Nat. Science † | | | | | | | | | | | | * |
| Phil. Philos. Soc.† | | | | | | | | | | | | * |
| Providence Athenæum † | | | | | | | | | | | | * |
| Brown University | | | | | | | | | | | | * |
| Yale College‡ | | | | * | * | * | | | | | | * |
| Harvard College† | | | | | | | | | | | | * |
| American Academy † | | | | | | | | | | | | * |
| Boston Nat. Hist. Society† | | | | | | | | | | | | * |
| Boston Athenæum | | | | | | | | | | | | * |
| Montreal Nat. Hist. Society † | | | | | | | | | | | | * |
| Super't Education, Montreal † | | | | | | | | | | | | * |
| Smithsonian Institution † | | | | | | | | | | | | * |
| Lyceum of Nat. Hist. N. Y.† | | | | | | | | | | | | * |
| Maryland Institute † | | | | | | | | | | | | * |
| Maryland Historical Society † | | | | | | | | | | | | * |
| Sold | 2 | 2 | 3 | 1 | 3 | 1 | 5 | 5 | 5 | 6 | 7 | 13 |
| To Members | | | | | | | 1 | 1 | 2 | 9 | 49 | 362 |
| Total | 2 | 2 | 3 | 2 | 4 | 2 | 6 | 6 | 7 | 15 | 56 | 393 |

*List of European Institutions to which Copies of Volume XII. of the Proceedings of the American Association were distributed by the Permanent Secretary in* 1859.

*Stockholm*, — Kongliga Svenska Vetenskaps Akademien.
*Copenhagen*, — Kongel. danske Vidensk. Selskab.
*Moscow*, — Société Impériale des Naturalistes.

* Sold.
† By order of the Association.
‡ Partly in exchange for rarer volumes.

*St. Petersburg*, — Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.
*Amsterdam*, — Académie Royale des Sciences.
*Haarlem*, — Hollandsche Maatschappij der Wettenschappen.
*Berlin*, — K. P. Akademie der Wissenschaften.
*Breslau*, — K. L. C. Akademie der Naturforschen.
*Franckfurt*, — Senckenbergische Naturforschende Gesellschaft.
*Freiberg*, — Koniglich Sächsische Bergakademie.
*Leipsic*, — Königlich Sächsische Gessellschaft der Wissenschaftan.
*Göttingen*, — Königlich Gessellschaft der Wissenschaften.
*Munich*, — K. B. Akademie der Wissenchaften.
*Prag*, — K. Böhm Gesellschaft der Wissenschaften.
*Stuttgart*, — Verein für Vaterländische Naturkunde.
*Vienna*, — K. Akademie der Wissenschaften.
" K. K. Geographischen Gessellschaft.
" Geologischen Reichsanstalt.
*Bern*, — Allgemeine Schweizerische Gesellschaft.
*Basel*, — Naturforschende Gessellschaft.
*Geneve*, — Société de Physique et d'Histoire Naturelle.
*Neuchatel*, — Société des Sciences Naturelles.
*Bruxelles*, — Académie Royale des Sciences, &c.
*Liège*, — Société Royale des Sciences.
*Lille*, — Société Nationale des Sciences, de l'Agriculture et des Arts.
*Paris*, — Institut de France.
" Société Philomatique.
*Dijon*, — Académie des Sciences, &c.
*Turin*, — Accademia Reale delle Scienzie.
*Madrid*, — Real Academia de Ciencias.
*Cambridge*, — Cambridge Philosophical Society.
*Dublin*, — Royal Irish Academy.
*Edinburgh*, — Royal Society.
*London*, — Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.

## BALANCE OF STOCK.

| Volumes | I. | II. | III. | IV. | V. | VI. | VII. | VIII. | IX. | X. | XI. | XII. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Balance March, 1859, | 59 | 95 | 256 | 237 | 443 | 301 | 538 | 869 | 964 | 911 | 918 | |
| Received from Binders, | | | | | | | | | | | | 1441 |
| Received from others,* | 1 | | | | | | | | | 1 | | |
| Total, | 60 | 95 | 256 | 237 | 443 | 301 | 538 | 869 | 964 | 912 | 918 | 1441 |
| Delivered as on page 870, | 2 | 2 | 3 | 2 | 4 | 2 | 6 | 6 | 7 | 15 | 56 | 393 |
| Balance, | 58 | 93 | 253 | 235 | 439 | 299 | 532 | 863 | 957 | 897 | 862 | 1048 |
| Delivered as on page 871, | | | | | | | | | | | | 48 |
| Balance March, 1861, | 58 | 93 | 253 | 235 | 439 | 299 | 532 | 863 | 957 | 897 | 862 | 1000 |

## REPORT OF THE AUDITORS.

This certifies that we have this day examined the above account of the Permanent Secretary, comparing the entries with the Treasurer's account and with the receipt-book, and the debits with the several vouchers, and find the whole correct, and the sum of five hundred and eighty-eight dollars and eighty-five cents $588.85 credited in the next account.

(Signed) A. D. BACHE,
J. W. FORSTER.

SPRINGFIELD, August 3, 1859.

* In exchange for volumes lost sent.

## REPORTS.

The Committee on Arctic Explorations, appointed at the Baltimore Meeting of the Association, report that they have attended to the duties intrusted to them, and have coöperated with Dr. I. I. Hayes in his efforts to obtain means to fit out another expedition to the Arctic regions. They would express their increased interest in the subject from recent developments, furnished by the discussion of Dr. E. K. Kane's meteorological observations, and would ask to be continued.

By order of the Committee,

A. D. BACHE, *Chairman.*

SPRINGFIELD, August 5, 1859.

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## CORRESPONDENCE.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: — It will be remembered by the Association, that a method was proposed by Dr. C. S. Gerling in 1847, whereby the solar parallax might be deduced from observations upon the planet Venus, near her inferior conjunctions, without awaiting the transit of the planet to take place in 1874; that the proposition was very generally discussed by American astronomers, and that, though some of them then doubted its practicability, the majority united in recommending an expedition to the southern hemisphere for the purpose of giving it a fair trial, and for making a series upon the planet Mars near the oppositions of that body.

The expedition having been authorized by Congress, departed from the United States in 1849, and was absent three years. On its return, its observations were placed for discussion in the hands of Dr. B. A. Gould, an astronomer well known to be eminently qualified for the task, and one whose absence from the United States in 1847 left him uncommitted upon the merits of a result from the special method.

His elaborate memoir preceding the mass of observations, published in Vol. III., U. S. N. Astronomical Expedition, is conclusive, and no

one can now doubt that the distance of the sun from the earth may be ascertained by corresponding observations in two hemispheres, made as were those in Chili.

Why did the expedition fail to determine this distance more accurately? This question has been a subject of much thought to me. Not thought productive of self-mortification or pain for my own want of zeal, or energy, or skill in manipulation, but of earnest regret that astronomers of the northern hemisphere, and more especially some of those in my own country — regardless of the claims due to the trial of a method so strongly recommended — should, by their neglect to make the corresponding observations, tacitly commit an act of discourtesy to co-laborers who had solicited their help. Reflect about it as I may, the conclusion is always the same; for the proposition went to them from Bache, the Bonds, Peirce, Walker, and others, who justly merited their confidence.

In support of the preceding conclusions, it may be proper to quote from two of many letters before me. In the first Prof. Peirce wrote *of* me: "He has demonstrated the possibility of a new and untried method of its (the solar parallax) determination, and if the requisite observations had been made by the astronomers of the northern hemisphere, with all the appliances so readily at hand, with the same fidelity and skill which are manifested in his own observations, conducted under much difficulty at his temporary station, the solution would have been all which could be desired. . . . . How greatly would it not redound to the . . . . . advancement of science if he . . . . . were directed to undertake a new determination of the parallax, with such ample powers and means as would secure the proper coöperation in the northern hemisphere, and enable our American astronomer to have the honor of permanently establishing the value of this great basis of astronomical measurement, and fixing the vastest of all the recognized standards of length."

Dr. Gerling wrote *to* me: "I must confess to very mingled sensations after examination of the volume. On one hand, I am compelled to admire the energy, assiduity, and zeal with which you prepared and carried out a work especially directed to the fair trial of a method that gave hope of success, and for which you had provided all astronomers with the ephemerides computed for your own use; and on the other, I am deeply grieved — *to use no stronger term* — that none were found

to support you with a sufficient number of corresponding observations. Your appeal to astronomers seems to have been mainly unheard or neglected; but it is not those of America only (as Peters indicates in No. 1177, Astr. Nach.) who are responsible, for in my judgment — and so I have written to him — the blame falls equally on the Europeans who had the means to coöperate with you. But, whilst I earnestly sympathize in the grief which I know the failure of this principal question will occasion you, I am made happy by the conviction that the other rich materials you have gathered for science have won for the expedition a glorious praise for all time. Moreover, I venture to hope that the problem of parallax, originated by the expedition, will soon be resumed, with proper means to *insure* corresponding observations in the manner you pointed out, and that your influence in both hemispheres will be sufficient to bring about another investigation which shall finally solve the question."

So many other correspondents concur in the propriety of another effort to obtain observations from which to determine the solar parallax, that I can no longer remain inactive. They fully coincide with me in the opinion, that if the question is to be solved during the present generation, it must be done by Americans. The late expedition was emphatically brought before our learned societies and Congress as *American*, and the results to be obtained by it, as constituting a contribution eminently worthy from our great nation to the old world, whose science we had so long appropriated without greatly augmenting. And so marked was the impression made abroad by the special claim for it, that warning came to me from Europe in the following language:

"I do not think you can calculate upon any coöperation except that of your own countrymen. All our observatories are engaged in their own work. Paramatta has no observer; Madras has lost Taylor; Maclear is too near Chili, and besides has his hands full. The English observatories have each their own line, and would not and could not derange themselves, except for some scheme which promised more profit. Greenwich will probably give you a fine series of Venus (for the planets belong to her province), but I do not think you are likely to get much more. Neither do I think it would be desirable. The expedition is so purely national and you have put forward its national character so strongly, that I would not, in your case, wish any help. If the thing is to be done, you have the means to do it (except heliometers), and must take it wholly upon yourselves."

Thus England, with observatories already established and first-rate instruments erected in opposite hemispheres, abandoned the field to us in 1849, for others, perhaps, that "promised more profit;" and, though Dr. Gould's discussion of the Santiago observations had not then reached them, from the paper read before the Astronomical Society two years ago we may safely conclude, that the astronomers royal, at Greenwich and the Cape of Good Hope, are no more ready now to derange their routine than they were then. No other nations can undertake it.

These facts justify me in presenting the subject to all who are jealous of our national fame or emulous of our scientific progress, and I am especially warranted in bringing it before that body whose title assures me of both. In so doing, I shall discuss, somewhat in detail, the two principal questions which bear directly upon the sought result. These are:

*First.* What probability is there of a more accurate determination of the sun's parallax?

The constant now in use is derived from the transits of Venus in 1761 and 1769, when the result from the former depended upon the accuracy with which the longitudes of distant stations were known, — a problem of much uncertainty even at the present day, — and the latter, to some extent, upon the observations of one who is now known to have forged his published data. All the observations were learnedly discussed by Prof. Encke, who says of this method: "In comparison with that of 1769, the next two transits (of Venus) will be so unfavorable that nothing short of perfection in the construction of instruments and art of observing can be expected to compensate for it."

The paper of the astronomer royal, at Greenwich, — already referred to, — leads to the same general conclusion. Both the transit of 1874 and that of 1882 will take place in December of the respective years. If we wish to avail ourselves of the method "by difference of transits," the phenomenon of 1874 will require observers in Siberia, Tartary, and other arctic regions; at that of 1882, they must occupy either the antarctic continent or the ice bounding its shores. Although summer will reign at the latter, who that reads the narratives of Captains Wilkes or Ross will be likely to make a voyage thither upon the slender contingency of landing at a fixed date? Will Siberia be more likely to attract observers in the winter of 1874? If not, the mode is impracticable, and so I am compelled to regard it.

If we would deduce the parallax from "the difference of observed times," it will be necessary to determine with precision the longitudes of stations now unknown, but which will be occupied; a problem already said to be one of difficulty, and, moreover, one which will inevitably involve a greater expenditure of time and money than several expeditions like the late one to Chili. Suppose, however, that the longitudes have been properly ascertained; in order to give additional precision to the accepted value, Prof. Encke tells us that the probable error of a single observed contact being seven seconds of time, the precision is only to be attained by having an immense number of observers at each station! So rare and important a phenomenon will unquestionably find many volunteers for its observation, and they will proceed to their distant posts well prepared for the work, if the means for their conveyance can be obtained. Then, their success will depend upon their having a clear sky during certain hours of that day. To how many of them, in opposite hemispheres, will those few hours prove favorable? If cloudy in either hemisphere, the costly preparations and tedious voyages will have been in vain. Weighing all these circumstances, the legitimate inference from Dr. Airy's paper corroborates the assertion of Encke, and assuredly we have the least possible amount, if any thing at all, to hope from transits of Venus prior to that of the year 2004.

The remaining methods are "by displacement of the planets Venus and Mars in declination," measured nearly simultaneously from observatories widely apart yet near the same meridian, and "by observing the displacement of Mars in right-ascension, when he is far east of the meridian and far west of the meridian as seen at a single observatory." For given reasons, Mr. Airy expresses a preference for the latter method; the former was followed by the late astronomical expedition.

The method by Venus when near her stationary terms, suggested by Dr. Gerling in 1847, has never been fairly tried. Most observers appear to have concluded that she is always too bright or too badly defined, because they have generally found her so in the evening when the refraction is most rapidly changing; but any one who will examine the morning differentials made at Santiago, and the remarks accompanying them, will be convinced that micrometrical measurements may be made with entire satisfaction, after the conjunction of the planet. At least, such was the case in Chili, and I doubt not that good morning

measures may be made elsewhere, more especially on elevated plateaus or where there is but little humidity in the atmosphere. Until the contrary is conclusively ascertained, Venus deserves a further trial, and particularly by observations on high ground.

The method "by displacement of Mars in right-ascension" is not a recent suggestion. It will be found in my correspondence with astronomers, which was published as a part of the Report from the Naval Committee when the Astronomical Expedition was recommended to Congress, in 1848. The precise mode of operation pointed out by Mr. Airy was carried into effect by Mr. W. C. Bond in 1849, and his observations, then made, form part of Vol. III. U. S. N. Astronomical Expedition. He also made a second series, in a manner which, to my mind, is likely to afford a far preferable result; for there is no known mode of registering transits over wires, from which a differential right-ascension can be determined so accurately as the micrometer screw will afford, when the equatorial is driven by a good clock. Leaving out of the question the comparative probable error from a concluded transit and a micrometrical measurement, as well as the discrepancies in the actual interval between evening and morning observations likely to arise from uncertain clock errors, Mr. Airy's mode involves, *first*, a knowledge of the very distance we are in search of; and *second*, that the plane of the instrument remains unchanged between the passages of the two objects over the wires. The former objection is not actually insuperable; but unless all parts of the telescope be made strictly homogeneous or the temperature remains unchanged between the transits of the two objects, the instrumental stability is more than doubtful.

Let us admit that good observations may be made *every* evening; how many times during the opposition period can we expect a clear atmosphere on the following morning? In such *summer* climates as has Chili and perhaps most of the elevated table-lands on the *western* side of our continent, there will be a very large proportion; probably seven or eight tenths of the whole would not be too many to expect. But, assuredly, at the old-established observatories, there will not be more than one in three. And thus, if the period be limited to the four weeks named by Mr. Airy, each observatory *may* have about ten nights of comparative results. Nevertheless, during the whole four weeks, as the observations will be valueless unless made on a consecutive evening and morning, or *vice versa*, the same observer must be at his post every

night, or personal equations, with their multiform and ever-varying phases, inevitably enter the discussion. I have endured a longer term of consecutive nights than that, though even under my regimen of life, — systematic as machinery, — it required a month of total relaxation to fit me for work again. This method is yet untried to any extent, but as it is urged by one who is justly enrolled among the most learned of Europe, and who receives our confidence, it is incumbent upon us to neglect no proposition which even promises a more reliable solution of the problem.

On the other hand, the method "by displacement in declination" has been found to be eminently practicable. The differential measures admit of the nicest accuracy, and of infinite repetition for the elimination of accidental errors. They may be made from the rising of the planet (Mars) to his going down, during which period a series for the previous method may be included. Observers may be changed *ad libitum* (except in the series for the latter), and thus, by the continuous interval embraced, it affords a greater probability for results from corresponding measures in opposite hemispheres, than from the night and morning observations of any one observatory.

At the oppositions of Mars in 1860 and 1862 he will be in a far more favorable distance from the earth than he was during any measurements in 1849–50 and 1851–52, or than he will be again until the summer of 1877. At neither period of my observations did the total measured displacement in declination exceed 14″.8, whilst next year and in 1862 it will be 22″.0, an advantage so important that, — with the certainty of fifteen years longer to doubt all celestial distances if we do, — no trifling obstacle should deter us from laboring to seize it.

*Second.* How many observatories will now coöperate to determine the sun's parallax more accurately?

There is no doubt that those of Washington, Albany, Cambridge, and Ann Arbor, — all of them provided with instruments of the first class, — will furnish full series of observations, and, for an object which so certainly promises success, we may also reasonably expect valuable data from amateur astronomers. Copies of the volume containing the observations made during the years 1849–52 were sent to all astronomers known to me, as well in this country as in Europe, and those who doubted the practicability of the method, when it was proposed in 1847, no longer have that as apology for withholding their aid. So earnestly

have they been blamed for their neglect to coöperate and thereby permit the principal object of the expedition to fail, that they would scarcely again venture to hold back. Certainly they would not do so in the United States, and probably not in continental Europe. It would not be so safe to promise for England. Even had not the astronomer royal recommended every establishment to make measurements of the displacement in right-ascension for itself, they may still "have each their own line to follow." But we need neither their aid nor that of any European observatory. America began the work — she is able to perfect it! We have ample means and power for doing so at the northern terminus, and I cannot believe that science will ask in vain the small sum necessary to secure corresponding observations from the southern extremity of our continent. Unfortunately for astronomy, there are only two observatories in the southern hemisphere. Of these, one, at the Cape of Good Hope, is English, and the other, at Santiago, is the national observatory of Chili. The latter has the instruments taken thither by the late astronomical expedition, to which its permanent foundation is due.

The director of the Cape observatory very cordially united with us in *one* of the series of observations on Mars, and his contribution forms part of the volume already referred to. He may be willing to do so again. If able to do so, the director of the Santiago observatory will, undoubtedly, carry out any plan of operations which may be proposed; but the government there is just completing a new observatory building and the instruments may not be ready in time to take part in the observations next year. Moreover, unless the director obtains competent assistance, his health is too delicate to resist the continuous nightly work that will be required. It appears almost certain, then, that if the problem is to be solved within our day, it must mainly if not altogether depend upon another expedition.

A government expedition is impracticable. The observations should extend through at least two oppositions of the planet Mars, and I have already said that the two most favorable ones prior to 1877 will be those of next year and of 1862. There remains only five months in which to construct and ship observatories and instruments to a distance of 5,000 miles, and prepare the necessary ephemerides. The executive would scarcely authorize the measure without the sanction of Congress, and the course of legislation by that body is such that a law could not be passed in time.

Nor can we expect pecuniary aid from the scientific societies of our country. None of them have the means at command to meet such expenses. And yet they will not exceed the following sums:

| | |
|---|---|
| 1. Observatories and instruments, . . . . | $6,500 |
| 2. Salaries of astronomer and assistants three years, . | 16,500 |
| 3. Travelling expenses and incidentals, . . . | 4,500 |
| 4. Freight and transportation, . . . . . | 2,500 |

Of the $30,000 needed, at least $8,000, — to arise from the sale of the instruments and buildings at the end of the work, — should be brought back to the United States to meet the expenses for calculations and publication of results.

The selection of a proper station at which to make the observations is of the first importance. It should be as far south as is consistent with climate, resources for the party, and its ulterior advantages to the cause of astronomy. Taking up these desiderata in reversed order, I do not hesitate to point out the Argentine Confederation as the most suitable country. Its high lands have a climate as favorable for observations as that of Chili, to which they are just near enough to excite emulation between the respective astronomers. Its people make daily progress in science and literature as well as the more immediately useful mechanic arts. Its present President has established peace and order where civil war and anarchy before prevailed, devoting the seven years of his administration to the promotion of measures calculated to secure confidence and respect for his government abroad, whilst organizing institutions that would elevate the standard of intelligence, and thus render to liberty a permanent home. Nor has he been less thoughtful for the material progress of his countrymen; so that within the next quarter of a century the States of La Plata will have so increased in population, enterprise, and wealth, as to take rank among the first nations of the world.

An observatory east of the Andes is greatly needed, and we may be morally certain that a national establishment will result as did the one in Chili, if an expedition for a similar object and equally well equipped, be sent to the sister republic.

Until the astronomer shall have made personal examination at Rasario, Paraná, San Luis, and Mendoza, and also has learned the wishes of President Urquiza, its actual locality cannot be decided upon.

In case of accidental injury to the instruments, for the sustenance of the party, or for the illness of either of them, they must be in or near a city. In choosing one, if it can be done without materially shortening the base of operations, it will be advantageous to approach the meridian of the northern observatories.

From the information now before me, Mendoza best satisfies these conditions, and therefore appears to be the most eligible place. It is only half a degree of latitude north of Santiago de Chili. It is 2,500 feet above the sea level. Its climate is probably much more favorable than that of towns in the valley of the Paraná, whilst its population of 9,000 souls are guaranty of the mechanical and other resources which may be needed. As San Luis is thirty-five miles further south than Mendoza and is quite as elevated as is that city above the ocean, it would be preferable to Mendoza were it not that the latter is 2° east of our most eastern observatory, and San Luis is 2°.30′ still nearer to the Atlantic.

And now, having presented the subject to the Association, I most respectfully submit that the occasion is one which entitles it to your support. In such case, it is believed that your appeal for funds to the patrons of science in the United States will not be made in vain, and I beg leave to suggest that they be requested to send the sums they will contribute to the Smithsonian Institution. That Institution can more properly be charged with the supervision of the work than any other, because of its national charter and of the fact, that, as the President of the United States is by law its Patron, his signature to the credentials of the astronomer would be of paramount importance in securing indispensable facilities and protection to the expedition.

J. M. Gilliss.

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Healing Springs, Va., 30th July, 1859.

Dear Sir, — Your kind and acceptable letter of the 16th, after following my wanderings through the mountains for some time, found me here two days ago, which will account for the want of that promptness in its answer which it deserves. I regret extremely that I am compelled to forego the pleasure of accepting your kind invitation to attend the meeting of the Association for the Advancement of Science, to be held at Springfield early in the ensuing month. Nothing would give me

more pleasure, but the state of my health is such that it forbids the thought of taking any trip, even one promising so much of pleasure and enjoyment as that to Springfield.

Present my most respectful acknowledgments to "the Association and citizens of Springfield" for their offer of "a cordial welcome" to me, and express to them my real regret at my inability to accept it.

With high esteem and respect, I am, dear Sir,

Very truly yours,

JOHN B. FLOYD.

MR. GEORGE A. OTIS.

HARTFORD, July 25, 1859.

SIR, — I have had the honor to receive through you an invitation from the Local Committee of the American Association for the Advancement of Science, to attend the meeting of the Association, which will be held at Springfield early next month.

Highly appreciating the objects of the Association, and the advantages resulting from its inquiries, I should take great pleasure in being present on the occasion, but my engagements will not permit it.

Allow me to express, through you to the Committee, and accept also for yourself, the acknowledgments due for this act of courtesy.

With sentiments of high respect,

I am very truly yours,

ISAAC TOUCEY.

DR. GEORGE A. OTIS, Jr., *Local Secretary, &c.*

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## INVITATIONS.

PALMER, August 4, 1859.

GENTLEMEN, — I have the honor of extending the courtesies of this Institution to your Association, and shall be most happy, on your excursion to Amherst, to show you one of those *noble State charities* for which Massachusetts is so highly distinguished.

Most respectfully yours,

JOHN M. BREWSTER, Jr.,

*Superintendent of the Monson State Alms House.*

TO the AMERICAN SCIENTIFIC ASSOCIATION.

## ERRATA.

Page 7, twenty-third line from top, for *ability* read *utility*.

Page 15, fourteenth line from top, for *found* read *formed*.

Page 19, eighteenth line from top, for *experiments* read *equipments*.

Page 23, twenty-sixth line from top, for *Guinard* read *Guinand*.

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